THURSDAY, FEBRUARY 14, 1895.

## *ELECTRIC OSCILLATIONS.*

*Les Oscillations Électriques.* Par H. Poincaré, Membre de l'Institut, Redigées par M. Ch. Maurain. (Paris: Georges Carré, 1894.)

We have already noticed at some length M. Poincaré's works on Électricité et Optique, and given an account of his views regarding Maxwell's theories, and his mode of presenting the subject in the light of Hertz's researches. The present work discusses more particularly the subject of Electric Oscillations, and includes comparatively little of the more abstract treatment of electrical theory to be found in the others. We have in it a very instructive and well-timed *résumé* of a good deal of the later work on the subject, with mathematical investigations of many points arising in connection with the various experimental researches referred to, which are of great value.

In a short preliminary statement of the theory underlying the Hertzian experiments, M. Poincaré adopts, to a certain extent, the mode of presentation used by Hertz himself. The two reciprocal sets of equations also made by Heaviside the starting-point of his researches, and called by him the circuital equations, are made by Hertz the basis of everything. Thus if L, M, N, X, Y, Z be the components of the magnetic and electric forces, $\mu$ and $\epsilon$ the "coefficients of magnetic and electric induction," and A the reciprocal of the velocity of light, the equations are

$$A\mu\left(\frac{\partial L}{\partial t}, \frac{\partial M}{\partial t}, \frac{\partial N}{\partial t}\right)=\left(\frac{\partial Z}{\partial y}-\frac{\partial Y}{\partial z}, \frac{\partial X}{\partial z}-\frac{\partial Z}{\partial x}, \frac{\partial Y}{\partial x}-\frac{\partial X}{\partial y}\right),$$

$$A\left(\epsilon\frac{\partial X}{\partial t}+4\pi u, \text{ \&c.}\right)=\left(\frac{\partial M}{\partial z}-\frac{\partial N}{\partial y}, \text{ \&c.}\right)$$

where $u, v, w$ are the components of conduction current at $x, y, z$. When $u, v, w=0$, one set of equations can thus be formed from the other set by interchange of L, M, N with X, Y, Z, and $\mu$ with $-\epsilon$. The electric energy is $\epsilon(X^2+Y^2+Z^2)/8\pi$, and the magnetic energy $\mu(L^2+M^2+N^2)/8\pi$, each taken per unit of volume of the medium.

The units of force are so chosen that $\mu$ and $\epsilon$ are both unity for vacuum, that is, for ether. Hence the introduction of A in the equations above. It is required to make the dimensions of both sides alike. We must confess that we much prefer the idea contained, though not clearly expressed, in Maxwell's treatise, of regarding $\mu$ and K as quantities dependent on the physical properties of the medium, and such that the velocity of light in the medium is $1/\sqrt{\mu K}$. There can be no question of the enormous advantage of thus proceeding. It avoids the obvious absurdity of giving in a general system of measurement founded on the units of length, mass, and time, two different sets of dimensions to the same quantity according as it is measured electrostatically or electromagnetically. Further, if this were recognised, the serious difficulty which besets students, when they find that what is defined or explained as a mere ratio is unity, or the reciprocal of the square of the velocity of light, according to the system of units adopted, would entirely disappear. The discussion which took place in 1882 regarding the dimensions of magnetic pole in electrostatic units, gave illustrations of the confusion and misconception to which neglect of the dimensional relation between $\mu$ and K can give rise in the mind of even a master of physical science.

Let $\mu_0, K_0$, or better, $\mu_0, \kappa_0$, be the inductive capacities (magnetic and electric) of ether, $\mu, \kappa$, those of any other substance, then $\mu/\mu_0$, $\kappa/\kappa_0$ will be the magnetic and electric permeabilities of the medium, and will be mere ratios. The magnetic permeability will then be as Lord Kelvin originally defined it, the ratio of two magnetic forces, of the magnetic force defined electromagnetically (in a crevasse across the direction of magnetisation) to the magnetic force according to the polar definition (in a narrow cylindrical hollow parallel to the magnetisation), and our ideas will no longer be liable to obfuscation in an important fundamental matter.

M. Poincaré derives the second set of equations stated above from the first set and the principle of conservation of energy, using for the total energy per unit of volume the sum of the expressions quoted above. The theoretical basis consists thus of (1) the experimental fact that the line-integral of electric force round a circuit is equal to the time-rate of variation of the total magnetic induction through the circuit, which gives the first set of equations; (2) the expressions for the magnetic and electric energies; and (3) the theorem that the work spent in heat per unit of volume per unit of time is $Xu+Yv+Zw$. The expressions for the energies are thus taken as fundamental. The result does not, however, depend on any supposition as to exact localisation of the energy; all that is involved is the expression of the energy as an integral extended through the whole field.

This is not the same as the process of Hertz, and it is not clear that it possesses any advantage over that method. In fact, Hertz's foundation is the two sets of reciprocal equations, which are first postulated, then shown to be consistent with observed phenomena. Thus the whole structure rests on the equations, and Hertz is but little, if at all, concerned with the dynamical explanation of electromagnetic phenomena. This, on the other hand, Maxwell is continually; and one of the most remarkable chapters of his book is that in which he applies the method of Lagrange to give a dynamical basis to the theory of induction and electromagnetic action. This was a natural outcome of the other process, which he followed, of establishing his equations as far as possible directly from the experimental facts, and connecting them by a strong web of dynamical theory. It does not seem likely, moreover, that the minds of many of those who are eagerly seeking for some more intimate knowledge of electromagnetic action will be content with anything but a dynamical explanation of electrical phenomena, and one which shall elucidate, in some measure at least, the relation of the ether to ordinary matter, and the reason for the existence of the ponderomotive forces exerted in the electromagnetic field.

In chapter iii. M. Poincaré proceeds to the theoretical study of Hertzian oscillations, and discusses in the first instance the solution of the differential equations for the case of conductors situated in a dielectric. The first part of this does not call for special remark. X is taken

in the ordinary way as the real part of $(B - iC)e^{ipt}$ where $i = \sqrt{-1}$, and $p = q - i\gamma$. The value of $p$ is here misprinted $\gamma + iq$. The author then proceeds to find solutions for the case of an indefinitely extended dielectric, and throwing the equations into the form

$$L, M, N = \frac{\partial \xi}{\partial y} - \frac{\partial \eta}{\partial z}, \&c.,$$

that is introducing the vector-potential, he goes on to solve the resulting equations for X, Y, Z. Two solutions are obtained of the form

$$\xi = -A \int \frac{(u)}{r} dxdydz$$

where $(u)$ may have either of the values $u'$, $u''$, given by $u' = f(x', y', z', t)$, $u'' = f(x', y', z', t - Ar)$. Here $u'$ is the value of the $x$-component of the current at the point $(x', y', z')$ at time $t$, $u''$ the value of the same component at the same point at a time previous by the interval required for the light to travel from $(x, y, z)$ to $(x', y', z')$, $r$ being the distance between these two points. The latter solution is, of course, adapted to the case of propagation in space with velocity $1/A$. It is exemplified by application to the complete solution of Lodge's spherical vibrator. This, in the assumed absence of radiation of energy and frictional damping out of the electrical vibration, may be regarded, for points external to the sphere, as simply a time-periodic electric doublet; for its electrification may be imagined produced by the relative displacement through a small distance of two uniform spherical volume distributions, each of which acts as if its whole charge were situated at its centre. But the existence of radiation very materially affects the result, for if $r$ be the distance of any point from the centre of the vibrator, $a$ the radius of the sphere, $\psi$ a function which gives the electric forces by the equations

$$X = \partial^2\psi/\partial x\partial z, \quad Y = \partial^2\psi/\partial y\partial z,$$
$$Z = -A^2\partial^2\psi/\partial t^2 + \partial^2\psi/\partial z^2,$$

the direction of the axis of the vibrator being that of $z$ then

$$r\psi = Be^{r/2a}e^{-t/2aA}\cos\frac{\sqrt{3}}{2a}\left(r - \frac{t}{A}\right) + Ce^{r/2a}e^{-t/2aA}\sin\frac{\sqrt{3}}{2a}\left(r - \frac{t}{A}\right).$$

This expression is interesting as showing exactly the damping due to radiation in this case; that due to frictional generation of heat is neglected. As $1/A$ is the velocity of light, it will be seen that the radiation damping is exceedingly rapid. This illustrates the difficulty of "maintaining" electric oscillations; and has an important bearing on the interpretation of results obtained with resonators which are damped slowly as compared with the exciters.

Of course, the damping in complicated cases can be found when the total flow of energy through a spherical surface of large radius, surrounding the vibrator, can be computed by Poynting's theorem. Thus it is possible to construct a more accurate solution for points distant from the vibrator, than that given by the supposition of no damping, by thus calculating approximately the exponential factor, and we can go on by successive approximation if need be.

The results obtained by Hert with regard to the period and the damping out of the vibrations of his exciter were tested, of course, by his resonator; and we have, in chapter iv. of M. Poincaré's work, an excellent account of the mathematics of resonance, so far as here required, and of the propagation of electric waves along wires, illustrated by reference to the experiments of Sarasin and De la Rive, Blondlot, and others. Many of these have reference to the variation of the current with distance from the end of the wire, at which, of course, reflections took place. The theory, for example, of Mr. D. E. Jones' experiments, in which a thermo-electric couple was used to measure the heating effect at different distances from the end of a long wire, is worked out; and the results, when compared with those of the experiments, are found to agree.

Chapters follow on Propagation in Air, and Applications of Theory. In the latter, M. Poincaré refers, among other things, to the results of the very important experiments of Bjerknes, on waves received by resonators of different metals; that is, their different rates of damping, and the depths to which the currents penetrate into the metal as measured by the thickness (to take an example) of copper, which must be deposited on an iron resonator in order that it may behave like one of solid copper.

These three chapters have a very important bearing on the question of multiple resonance, to which M. Poincaré has himself devoted special attention, and form, perhaps, the most valuable part of the work.

Lastly, the book deals with such applications of electrical oscillations as the determination of specific inductive capacities, the reflection and refraction of electric waves, and ends with a study of Hertz's "Memoir on the Fundamental Equations of Electrodynamics for Moving Bodies."

The mathematical discussions are, as we have indicated, throughout lucid and very frequently elegant. The facility with which an apparently intractable problem is taken hold of, and a few more or less elementary considerations made to yield an approximate solution or result, is very remarkable. We could have wished for a somewhat more physical treatment of the subject; but to those whose physical ideas have had some training with regard to these matters, M. Poincaré's treatise cannot but be of great service, as supplying what no doubt was its object, a review of the principal results obtained in this field of research compared with theory, as far as possible by the only sure test, that of calculation.

A. GRAY.

## THE BOOK OF THE ROSE.

*The Book of the Rose.* By the Rev. A. Foster-Melliar, M.A., Rector of Sproughton, Suffolk. Pp. 336. 29 illustrations. (London: Macmillan and Co., 1894.)

MR. MELLIAR is well known among horticulturists as a successful grower and exhibitor of roses, and his book is what he wished it to be—the rose considered as a flower, with full details for its practical culture for amateurs, from the beginning to the end. Art is his text all through. He has very little to say about the botany of the rose, its geographical distribution, the origin of the numerous races of garden roses as distinguished from their wild progenitors, the fourteen chapters of his book

being devoted to such matters as soil, manure, planting, pruning, propagating, and exhibiting. For the cultivator the book covers all the ground, and Mr. Melliar's instructions are as clear, as thorough, and as trustworthy as any budding "Rosarian" could desire. There are already plenty of books about the rose, of which as much as this can be said, but there is still room for Mr. Melliar's. He writes mainly for the amateur grower of roses, and strongly recommends his hobby to the country parsons who are, he says, "all so poor, and likely to be poorer still." The delights of the rose-grower are only experienced when he himself does all the work entailed in the production of plants from cuttings, buds or seeds, to the exhibition flowers. The genuineness of Mr. Melliar's love for his hobby is seen in the following extract from his book :—

"But if it was you alone who had found, chosen, and grubbed out the [briar] stock from the hedge, or cut, prepared, planted, and transplanted the briar or manetti cutting - if no hand but yours had budded it, cared for it in all stages, and finally cut and shown the Rose—then, when perchance it is declared on all hands to be the finest specimen of the variety ever shown, it must be an additional pleasure to know that it is your Rose indeed, for that, as far as all human aid is concerned, you made it yourself."

If the book has a fault, it is to be found in this treating of the rose solely as a flower to be set on a tray at an exhibition and win a prize. Mr. Melliar has something of the narrowness of view of the old florist, for he sees little beauty in roses which are not likely to win prizes. He devotes thirty pages to instructions on exhibiting the flowers. This chapter is interesting as showing how much care and self-sacrifice are necessary to success in the exhibition tent, the paper covers to protect the buds from rain, wind, and sun, and the precautions necessary even when the flowers are ready to pack in their prepared mossy beds in boxes. In one very dry season, he says, his mossed boxes had been kept in the shade and duly watered, with the result that two huge slugs, each as big as his thumb, concealed themselves in the moss, and during the journey to the Crystal Palace regaled themselves on the roses!

Mr. Melliar speaks of several varieties of rose that show deterioration since they were first introduced, or "sent out," as the raisers express it. These fine varieties of rose are always propagated from grafts or cuttings, and deterioration is therefore unlikely. The same statement has been made with regard to apples, pears, and other fruits, which are multiplied in the same way as roses, but they have never been substantiated.

Until within the last thirty years or so, all the best garden roses were raised by continental growers, the belief being that they could not be produced in England. The same belief prevailed until recently with regard to chrysanthemums. It is therefore a pleasant surprise to see that of the 144 varieties of the Hybrid Perpetual section of roses, selected by Mr. Melliar as the choicest and best, no less than sixty of them were raised in England, and that the two best H.P. roses known, viz. "Her Majesty" and "Mrs. J. Laing," were raised by the late Mr. Bennett in Surrey. To these I would add a third, namely, "Grace Darling," also raised by Mr. Bennett, and which, although not an exhibition rose, is yet one of the most beautiful of all in the garden. Its only rival is "Gloire de Dijon," raised in France forty years ago, and of which Dean Hole, prince of rosarians said, "Were I condemned to have but one rose for the rest of my life, I should ask, before leaving the dock, to be presented with a strong plant of 'Gloire de Dijon.'"

A book which deals chiefly with exhibition roses can scarcely be considered to justify its title, "The Book of the Rose," because to all but a very small section of gardeners the rose appeals with greatest force when seen as a naturally grown bush in the garden. The close pruning and other rigorous methods of the grower-exhibitor are suggestive of ear-cropping for dogs, and comb-cutting for cocks, disfiguring, for a special purpose, otherwise beautiful objects. Mr. Melliar has very little to say for the many beautiful single-flowered roses which are now coming rapidly into favour with gardeners, and whose flowers are often as lovely as the highest floral art could wish. Many of the perfect exhibition roses are just about as attractive in form and colour as a red-cabbage. The hybrids raised by Lord Penzance from the sweetbriar crossed with garden roses, are exceedingly beautiful, so too are the forms of the Japanese *Rosa rugosa*, *R. multiflora*, *R. bracteata*, *R. indica*, &c.

It is becoming a too common practice amongst professional gardeners to set about crossing beautiful species of plants, roses included, with a view to doubling or otherwise altering the form, or "improving" the colour of the flowers, to satisfy some absurd ideal, the consequence often being a considerable loss from the point of view of true art. We owe the garden roses of the present time to the breeders of florists' flowers, and are duly thankful. At the same time we would prefer to keep the best of the single-flowered roses just as they are. Mr. Melliar does not agree with those who look upon the rose as a decorative plant for the garden, but only as a means whereby he may obtain glorious roses. For me, the best tray of exhibition roses ever produced has infinitely less charm than a bed of yellow Banksian roses, or even a tangled mass of sweetbriar when covered with flowers. However, Mr. Melliar's book will teach any one how to grow good rose flowers, and there are thousands who would find pleasure in rose-growing by reading and following his directions. W.

## OUR BOOK SHELF.

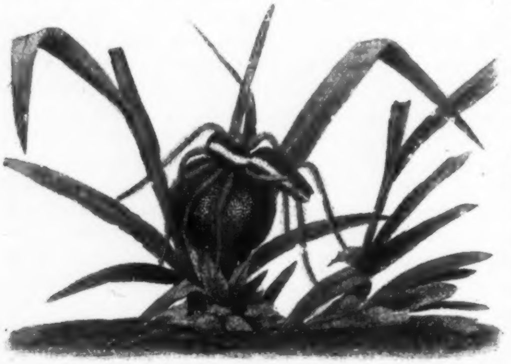
*L'Industrie des Araneina.* By Woldemar Wagner. (St. Pétersbourg: L'Académie Impériale des Sciences, 1894.)

THIS work may be divided into two sections: (1) the descriptions of the cocoons, nests, &c., of the commoner kinds of Russian spiders; and (2) the conclusions respecting the evolution of instincts and the classification of the species, to be drawn from the data thus established.

Wagner recognises four kinds of silken structures—namely, the snare, the retreat, the nest, and the cocoon. About the first of these but little is said. The remaining three, however, are discussed in considerable detail, and to the question as to whether these structures are of great taxonomic value, M. Wagner gives an unhesitating and most positive affirmative reply. It is unfortunately impossible within the limits of a short notice to criticise

closely his arguments; but that they are worthy of the deepest attention, will be realised by all arachnologists who are acquainted with M. Wagner's previous works.

Perhaps the highest praise that can be bestowed upon this work, is to say that, even when stripped of its theories, it quite reaches the high standard of excellence attained by M. Wagner's previous papers, and that the great value of the theoretical part lies in the fact that



the key to all problems is sought in the hypothesis of evolution by means of Natural Selection.

The memoir is admirably illustrated with ten lithographed plates, of which eight are coloured, and, in addition, with two hundred and fifty-two diagrams in the text. The figure we here select for reproduction, to give an indication of the nature of the rest, represents the female of *Ocyale mirabilis* carrying her cocoon.

R. I. P.

*Elementary Practical Chemistry, Inorganic and Organic.* By J. T. Hewitt, M.A., D.Sc., Ph.D., F.C.S., and F. G. Pope. Pp. 42. (London: Whittaker and Co.)

ALTHOUGH the authors of this small book have confined themselves to such parts of elementary qualitative analysis as find a place in Stage I. of the Science and Art Department Syllabus, neither in general plan nor in details of treatment does the book possess any educational advantage over its many competitors. A mere recital of reactions cannot be considered as "Elementary Practical Chemistry." Surely it is possible to present even the array of facts utilised in analysis in such a manner as to comply with the fundamental requirements necessary to be fulfilled by all educational works designed for young students of science. The production of compilations of the present type will probably cease to exist when the new regulations for Organised Science Schools come into force. We may then, perhaps, look for the production of really philosophical text-books arranged on sound educational lines, and yet calculated to minimise the very real difficulties encountered by the beginner. Putting aside these fundamental considerations, it is only just to say that the authors have brought together a strictly limited set of reactions with few positive inaccuracies.

*How to Live in Tropical Africa.* By J. Murray, M.D. Pp. 252. (London: George Philip and Son, 1895.)

SO far as literary merit is concerned, this is a poor book. The text is disjointed, it is too full of unnecessary quotations, and there is too much tautology. But if only the subject-matter is considered, the verdict is that the book is a trustworthy guide to tropical hygiene, and a useful manual on the cause, prevention, and cure of malarial fevers. The importance of such a handy volume to emigrants and visitors to Africa can hardly be overstated. And as the book is the outcome of medical experience, it possesses exceptional value.

## LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

### The Liquefaction of Gases.

PROF. OLSZEWSKI'S letter in NATURE of January 10 is a more serious matter than *a claim for priority.* The letter charges Prof. Dewar with allowing the impression to go abroad that he has carried out much original research into the methods of liquefying the more permanent gases, and the properties of the liquids produced; whereas, according to Prof. Olszewski, most of Prof. Dewar's experiments have been merely repetitions of work done by others.

In his brief communication to NATURE (January 10), Prof. Dewar has been too modest either to defend himself or to meet his opponent. Fortunately, he makes one definite statement:—"A reference to the *Proceedings* of the Royal Institution between the years 1878 and 1893 will be sufficient to remove the suggestion that the apparatus I use has been copied from the *Cracovie Bulletin* of 1890."

I have followed Prof. Dewar's recommendation, and made references to the *Proceedings* of the Royal Institution. In his lecture at the Royal Institution on June 13, 1884 (*Proc. R. I.*, xi. 148), Prof. Dewar refers to Messrs. Olszewski and Wroblewski as having "recently made such a splendid success in the production and maintenance of low temperature." He describes and figures an apparatus which is a slightly modified form of that of the Polish professors, which in turn was derived from the apparatus of Cailletet; and he says: "Provided a supply of liquid ethylene can be had, there is no difficulty in repeating all the experiments of the Russian observers." No claim is made here to originality in the essentials of the apparatus, nor in the experiments performed. The apparatus referred to by Prof. Dewar in his lecture of June, 1884, was used by Prof. Olszewski in 1883, and was improved by him in 1884; in 1887 the apparatus was made capable of liquefying oxygen and other gases in considerable quantities at the ordinary atmospheric pressure (see Olszewski, *Phil. Mag.* February 1895, 189–190). In 1890 the apparatus was so improved that from 30 to 100 c.c. of liquid oxygen could be produced by it (see Olszewski, *Phil. Mag.* February 1895. 192–193). Prof. Olszewski states (NATURE, January 10) that a description of this improved apparatus was sent to Prof. Dewar. A year after this, on June 26, 1891, Prof. Dewar delivered a discourse on Faraday's work at the Royal Institution. The published abstract of this lecture (*Proc. R. I.* xiii. 481) contains a photograph of the pumps and engines used in the laboratory of the Royal Institution, and a photograph of the arrangement of the apparatus on the lecture table; but it is impossible to make out the details of the apparatus from these photographs. So far as can be judged from the *Proceedings* of the Royal Institution, Prof. Dewar did not show large quantities of liquid oxygen, nitrogen, or air in his lectures until June 10, 1892, when he placed before his audience a pint of liquid oxygen. Two years before this Prof. Olszewski had obtained 100 c.c. of liquid oxygen, and he tells us in his letter to NATURE (January 10) that 200 c.c. of this liquid were prepared and exhibited by him in July 1891. A pint is undoubtedly more than 200 c.c., but unless one does something with the larger quantity which cannot be done equally well with the smaller, nothing is gained by conducting the manufacture on the large scale.

I can find no other mention of the apparatus used at the Royal Institution for liquefying large quantities of gases. There is indeed no accurate description in the *Proceedings* of that Institution of the apparatus used by Prof. Dewar. If Prof. Dewar has made marked improvements in any essential parts of Prof. Olszewski's apparatus, why has he not published an accurate description of these improvements in some recognised scientific journal?

A reference to the *Proceedings* of the Royal Institution is then sufficient, not to remove, but to strengthen, "the suggestion that the apparatus I [Prof. Dewar] use, has been copied from the *Cracovie Bulletin* of 1890," or at least that it has been borrowed from descriptions of apparatus devised by Prof. Olszewski.

Every one must admire, and praise the skill which fashioned, the vacuum receivers for storing and experimenting with comparatively large quantities of liquid oxygen and air, which Prof. Dewar used in public for the first time on January 20, 1893, so far as can be gathered from the *Proceedings* of the Royal Institution (see *Proc. R. I.* xiv. 1.) The following sentences occur in the published abstract of Prof. Dewar's lecture on that date: "The prosecution of research at temperatures approaching the zero of absolute temperature is attended with difficulties and dangers of no ordinary kind. *Having no recorded experience to guide us in conducting such investigations*, the best instruments and methods of working have to be discovered." (The italics are mine.) (*Proc. R. I.* xiv. 1.) Now, in June 1890, that is two and a half years before Prof. Dewar made the statement I have quoted, Prof. Olszewski was able to say: "My new apparatus excludes the inconveniences of the former one, and renders it possible to preserve the liquid oxygen a longer time under the ordinary atmospheric pressure." And again: "Thus was solved the problem of liquefying considerable quantities of oxygen without the slightest danger." (See Olszewski, *Phil. Mag.* February 1895, 193-3.) Every reader of Prof. Olszewski's paper will agree that these statements are justified. Moreover, in October 1891, that is, fifteen months before Prof. Dewar declared there was "no recorded experience to guide us in conducting such investigations," Prof. Olszewski (in conjunction with M. Witkowski) described a method for not only storing, but also experimenting with, considerable quantities of liquid oxygen (*Cracovie Bulletin*, October 1891). So far back as 1887 he published, in *Wiedemann's Annalen* (xxxi. 58), a full account of methods for determining the densities of liquid oxygen, nitrogen, and marsh gas. In 1887, also, he determined the boiling point of liquid ozone, and he began his measurements of the absorption spectra of liquid oxygen and air (*Wiedemann's Annalen*, xxxiii. 570); and in 1891 he published further measurements of the absorption spectrum, and the refractive index, of liquid oxygen (*ibid.* xlii 663; see also *Phil. Mag.* February 1895, 197-9, and 206-8.)

It is just these properties, namely, the optical properties, of liquid oxygen which had been elaborately studied by Prof. Olszewski from 1887 to 1891 that are largely dwelt on by Prof. Dewar in his discourse of January 1893; and it is the illustration of these properties that is prefaced by the remark, there is "no recorded experience to guide us in conducting such investigations."

On January 20, 1893 (*Proc. R. I.* xiv. 10) Prof. Dewar said that liquid oxygen is "the most convenient substance to use for the production of temperatures about $-200°$ C. Liquid nitrogen, carbonic oxide, or air can conveniently be made at the ordinary atmospheric pressure, provided they are brought into a vessel cooled by liquid oxygen boiling under the pressure of about half an inch of mercury." It is interesting to compare with this Prof. Olszewski's words published in June 1890 (*Crac. Bull.*; I quote from the translation in *Phil. Mag* February 1895, 192): "I proved long ago [*Compt. rend.* c. 350 (1885)] that liquid oxygen is the best cooling agent; for it easily gives the temperature of $-211°\cdot5$ C. if the pressure is lowered 9 mm. of mercury, and it does not freeze even at the pressure of 4 mm." It may be said that the similarity between these statements is merely a coincidence. Perhaps it is. Nevertheless, Prof. Dewar's statement conveys the impression that he was the first experimenter to employ liquid oxygen for obtaining, and maintaining, very low temperatures. Another of Prof. Dewar's statements which imply more than they express, occurs in his lecture of June 10, 1892 (*Proc. R. I.*, xiii. 695): "He hoped that evening to go several steps further, and to show liquid air, and to render visible some of its more extraordinary properties." Remembering that air had been liquefied by Prof. Olszewski at least eight years before the lecture wherein this statement occurs was delivered, one must regret that Prof. Dewar did not choose words which should have made it impossible for the English public to suppose "that the liquefaction of oxygen and other so-called permanent gases was achieved for the first time by [him]." (See Prof. Olszewski's letter in NATURE of January 10).

Not only does Prof. Olszewski claim to have devised the methods, and constructed the apparatus, for liquefying considerable quantities of the more permanent gases, but he also asserts that most of the experiments on the properties of these liquefied gases which have been performed by Prof. Dewar are only repetitions, on a larger scale, of work done by others. What, then, are these experiments?

Measurements of the refractive index of liquid oxygen, for the sodium light, and also of the absorption spectrum of liquid oxygen, were published by Profs. Liveing and Dewar in August 1892 (*Phil. Mag.* (5) xxxiv. 205; see also Dewar in *Proc. R. I.*, June 10, 1892; and Liveing and Dewar in *Phil. Mag.* for August 1894). The refractive index of liquid oxygen had been measured, and determinations of the absorption spectrum had been made, by Olszewski and Witkowski in October 1891, and these measurements were the complement of work begun, and published, by Prof. Olszewski in 1887 (*Wiedemann's Annalen*, xxxiii. 570. See Olszewski in *Phil. Mag.*, February 1895, 197-9). It is true that preliminary experiments on the absorption spectrum of liquid oxygen had been made by Profs. Liveing and Dewar in 1889. In their paper in *Proc. R. S.*, June 6, 1889, they say:—"We have observed repeatedly the absorption of liquid oxygen in thicknesses of 8 and 12 mm. Our observations confirm those of Olszewski."

On December 10, 1891, Prof. Dewar announced, in a note to the President of the Royal Society, that liquid oxygen is attracted by the poles of a magnet. This note was followed by another, on December 17, 1891, saying that liquid ozone was also attracted by the poles of a magnet. These detached, but interesting, experiments, which followed up work done by Faraday many years ago, must be placed to the credit of the Fullerian Professor at the Royal Institution.

A paper published in *Phil. Mag.* (5) xxxiv. 326 (1892) by Dewar and Fleming, on the electrical resistances of metals and other bodies at very low temperatures, contains the only piece of exact investigation to which Prof. Dewar's name is attached wherein liquid oxygen or air is used as an instrument of research, with the exception of the measurements of the optical properties of liquid oxygen, which have been shown to be mainly repetitions of the work of Olszewski and Witkowski. This research carries on and supplements earlier work done by Cailletet and Bouty, and by Wroblewski (*Comptes rendus*, ci. 161 (1885).

A few observations have been made by Prof. Dewar on the phosphorescence of various substances at the temperature of boiling oxygen, and on the cessation of chemical action, and the continuance of photographic action, at the same temperature (see *Proc. R.I.* June 26, 1891 and June 10, 1892, also January 20, 1893; also *Proc. Royal Soc.* April 19, 1894, and *Proc. Chem. Soc.* June 28, 1894). The subject of chemical action at very low temperatures was investigated by Pictet in May and November 1892 (*Comptes rendus*, cxiv. 1245; cxv. 814) in a much more exhaustive way than has been done by Dewar, who has only touched the fringe of the matter (compare also Olszewski, *Phil. Mag.* February 1895, 208-9). The observations on the sensitiveness of the Eastman film at very low temperatures are interesting; but, so far as it has been published, the work is too slight to present material for detailed criticisms; and the same may justly be said of the lecture illustrations of phosphorescence at low temperatures. It may be remarked that Prof. Dewar's paper on photographic action at low temperatures has appeared only in the *Proceedings*, not in the *Transactions*, of the Chemical Society.

Prof. Olszewski's work on the properties of liquefied gases was begun, and accounts of his accurate experiments were published in recognised scientific journals, long before Prof. Dewar's experiments were heard of. The work of the Polish Professor is being continued in the same exact, modest, and scientific manner (see his paper on the optical dispersion of liquid oxygen in *Cracovie Bulletin*, July 1894, noticed in *Phil. Mag.*, February 1895, 208; and his determinations of the boiling and freezing points, and other constants, of argon communicated to the Royal Society, January 31, 1895).

It seems to me that Prof. Olszewski has established a case which demands the instant and serious consideration of those who are truly interested in the advance of science, and are jealous of the good name of the scientific men of this country.

Cambridge, February 7. M. M. PATTISON MUIR.

THE object of the communications on the liquefaction of gases, which have recently appeared in NATURE and the *Philosophical Magazine*, is to depreciate the work of Cailletet and Pictet, to smother away the first-class work of the deceased

Wroblewski; to annihilate myself, and thereby to magnify the claims for originality of Prof. Olszewski. In spite of the mistakes inevitable to pioneers, the work of Cailletet and Pictet must always be credited with originating research in this department. To show my appreciation of such investigation, and to give it a wide publicity in the year 1878, a Friday evening address was devoted to the work of Cailletet and Pictet, during the course of which I showed for the first time in this country the working of the Cailletet apparatus. Similarly, in the year 1884, an address was given on the work of Wroblewski and Olszewski, in the course of which I illustrated for the first time in public the liquefaction of oxygen and air, showing the boiling point, &c., by means of a simple form of apparatus which did not involve the use of the Cailletet pump as employed by these experimenters. To deliver a lecture on the work of other people is generally considered a mark of honour, and as usefully diffusing a knowledge of the same. The critic has for some object omitted half the opening sentence of this address, which runs as follows :—

"The two Russian chemists, MM. Wroblewski and Olszewski, who have recently made such a splendid success in the production and maintenance of low temperature, have used in their researches an enlarged form of the well-known Cailletet apparatus; but for the purpose of lecture demonstration, which necessarily involves the projection on a screen of the actions taking place, the apparatus represented in the annexed woodcut is more readily and quickly handled, and enables comparatively large quantities of liquid oxygen to be produced."

The same tactics have been adopted of taking half a sentence from another part of the address, and leaving out the context. The sentence to which I refer ran thus :—

"*Provided a supply of liquid ethylene can be had, there is no difficulty in repeating all the experiments of the Russian observers;* but as this gas is troublesome to make in quantity, and cannot be bought like carbonic acid or nitrous oxide, such experiments necessitate a considerable sacrifice of time. It was therefore with considerable satisfaction that I observed the production of liquid oxygen by the use of solid carbonic acid, or, preferably, liquid nitrous oxide."

Then follows a full description of how to use nitrous oxide with the apparatus in order to get liquid oxygen. Note the disingenuous comment of the critic: "No claim is made here to originality in the essentials of the apparatus, nor in the experiments performed." It has never been the custom to make public claims for originality, either in apparatus or experiment, in the course of an address given at the Royal Institution. He proceeds to allege, "the apparatus referred to by Prof. Dewar in his lecture of June, 1884, was used by Prof. Olszewski in 1883." On turning to the reference given, I find the following statement by Olszewski: "In 1883, and for several years following, I liquefied the gases in a strong glass tube." According to this distortion of fact, the apparatus of Andrews, Cailletet, Wroblewski, and Dewar are all the same, because gas was liquefied in a glass tube. I challenge the production of a reference to an apparatus antecedent to the date of this lecture in which liquid nitrous oxide had been used for the production of liquid oxygen. But all this is ancient history; the real charge begins with the year 1890. Assuming that I had been the thief of Prof. Olszewski's apparatus of 1890, which is alleged to be the type used in the Faraday Commemoration Lecture of 1891, *what explanation is forthcoming of the delay of four years in ventilating the question of priority and personal grievance? What has occurred at the present juncture to precipitate a crisis?* Can any one avoid coming to the conclusion that Prof. Olszewski's contact with "Argon" explains the sudden assault upon my labours in the same field of investigation. . . .

If the critic had been a trustworthy person even to examine into the published facts, he could have found a reference in the *Proceedings* of the Royal Institution, between the years 1878 and 1893, that would have enlightened his ignorance, and thereby prevented his reiterating Prof. Olszewski's suggestion that my large apparatus of 1891, which produced pints of liquid oxygen, was copied or borrowed from a description in the *Cracovie Bulletin* for 1890. . . .

Let us see what Prof. Olszewski said in that paper, entitled "Transvasement de l'oxygene liquide," June 1890 :—

"A flask of wrought iron, five litres in capacity (such as is used to hold liquid carbon dioxide), containing oxygen under a pressure of 80 atm., is joined by a narrow copper tube to the upper end of a steel cylinder tested at a pressure of 200 atm. This cylinder having a capacity of 30–100 cub. centim., according to the quantity of oxygen which we wish to liquefy at a time, is immersed in liquid ethylene, of which the temperature may easily be lowered to 140 c. by means of an air-pump. The lower end of the cylinder is joined by a narrow copper tube to a little stopcock, through which the oxygen, liquefied in the cylinder, can be poured down into an open glass vessel, kept cool by the surrounding air."

In other words, replace the glass tube in my apparatus of 1884 by a small steel cylinder, and attach to its lower end a narrow copper tube with a stopcock, and the Olszewski apparatus of 1890 is produced (as a matter of fact, Pictet had used the same principle in the year 1878). Now, on June 11, 1886, I delivered a lecture on "Recent Researches on Meteorites," and the report in the *Proceedings* of the Royal Institution contains a sectional drawing of an apparatus solely constructed of copper, together with a valve for drawing off liquid oxygen, entirely different in type from the crude plan Olszewski adopted in 1890. I may mention that the plan of the apparatus was reproduced immediately after the delivery of the lecture, both in England and America. The section is confined to the refrigerator, all the accessories of liquefied and compressed gas bottles, compression and exhaust gauges, &c., having been omitted. From this plan of the refrigerator, any person so desiring could increase its capacity so as to work on a larger scale. The drawing shows the apparatus arranged for the special experiment of ejecting liquid oxygen into a vacuum chamber, but it is clear the apparatus discharged as easily into an open vessel. It is not any isolated experiment that is in dispute, but the new form of apparatus. The special object for which liquid oxygen was in use in this lecture, was to cool a piece of meteorite before insertion into an electric furnace. The following extract from the lecture will explain how the subject was introduced :—

"Meteorites, no doubt, have an exceedingly low temperature before they enter the earth's atmosphere, and the question had been raised as to what chemical reactions could take place under such conditions. It resulted from Prof. Dewar's investigations that at a temperature of about 130° C., liquid oxygen had no chemical action upon hydrogen, potassium, sodium, phosphorus, hydriodic acid or sulphydric acid. It would appear, therefore, that as the absolute zero is approached, even the strongest chemical affinities are inactive.

"The lecturer exhibited at work the apparatus by which he had recently succeeded in solidifying oxygen. The apparatus is illustrated in the accompanying diagram, where a copper tube is seen passing through a vessel kept constantly full of ether and solid carbonic acid; ethylene is sent through this tube, and is liquefied by the intense cold; it is then conveyed by the tube, through an india-rubber stopper, into the lower vessel; the outer one is filled with ether and solid carbonic acid. A continuous copper tube, about 45 feet long, conveying oxygen, passes through the outer vessel, and then through that containing the liquid ethylene; the latter evaporates through the space between the two vessels, and thus intense cold is produced, whereby oxygen is liquefied in the tube to the extent occasionally of 22 cubic centimetres at one time. The temperature at which this is effected is about – 130° C., at a pressure of 75 atmospheres; but less pressure will suffice. When the oxygen is known to be liquid, by means of a gauge near the oxygen inlet, the valve A is opened, and the liquid oxygen rushes into a vacuum in the central glass tube below; some liquid ethylene at the bottom of the next tube outwards is also caused to evaporate into a vacuum at the same moment, and instantly some of the liquid oxygen in the central tube becomes solid, owing to the intense cold of the double evaporation. The outer glass vessel serves to keep moisture from settling on the sides of the ethylene tube. By means of the electric lantern and a lens, an image of this part of the apparatus was projected upon the screen, this being the first time that the experiment had been shown on a large scale in public.

"Performing this experiment, the temperature reached was a little below 200° C., that is, only 50° or 70° above the absolute zero of temperature, and in the experiment about 5 lbs. of liquid ethylene were employed."

This I declare to be the first apparatus made wholly of metal, being an arrangement of copper coils, in which liquid oxygen

was made and decanted or transferred from the vessel in which it was liquefied to another by means of a valve, and thereby rendered capable of use as a cooling agent. In support of this assertion, I call as witness Prof. Charles Olszewski himself, who states in the *Philosophical Magazine*, February 1895, p. 189: "In 1883, and for several years following, I liquefied the gases in a strong glass tube." There is no suggestion made that a steel cylinder and valve was used by Olszewski till the year 1890. Whereas four years in advance I had used a much safer and better form of apparatus, practically identical in principle with that used in Cracow in the year 1890. Have I ever suggested that Prof. Olszewski was anticipated, or attempted to raise any question of priority? Perhaps the critic will have the audacity to say, in reply, this is no publication, the *Proceedings* of the Royal Institution, English and American science periodicals, not being amongst the class of recognised scientific journals. Well, if I am pleased to throw my bread upon the waters, adopting the view that every truthfully recorded experiment which appears in any journal associated with my name is publication, surely I should simply be conducting myself in the "too modest" way my critic commends.

As a specimen of the distortion of facts to prove another case of priority that is claimed, I find that MM. Charles Olszewski and Auguste Witkowski, Membres Correspondants, presenting their memoir "Propriétés optique de l'oxygene liquide," on October 3, 1892, and, on referring to the paper, it is dated July 15, 1892, and the following footnote is added:

"Avant la publication de notre communication, MM. Liveing et Dewar ont fait connaitre (*Phil. Mag.* Aout 1892), les resultats de leurs recherches sur la refraction des gaz liquéfiés."

Yet the critic says our experiments were "mainly repetitions of the work of Olszewski and Witkowski." The garbled extracts selected to make it appear that I have been guilty of misrepresentation are all of the same kind. . . . Thus I am taken to task for using the expression in the lecture on liquid air, of 1893: "Having no recorded experience to guide us in conducting such investigations, the best instruments and methods of working have to be discovered." The next sentence runs as follows: "The necessity of devising some new kind of vessel for storing and manipulating exceedingly volatile fluids like liquid oxygen and liquid air, became apparent when the optical properties of the bodies came under examination. Apart altogether from the rapid ebullition interfering with the experimental work, the fact that it did take place involved a great additional cost in the conduct of experiments on the properties of matter under such exceptional conditions of temperature." What can be said in defence of such glaring misrepresentation of the meaning of my words? Mr. M. M. Pattison Muir's demand for "*instant and serious consideration*" of his client's "*case*" has been quickly met. I trust the result will . . . . fit in with his brief.

JAMES DEWAR.

Royal Institution, February 12.

[A few personal remarks in Prof. Dewar's letter have been omitted, as they do not affect the points at issue.—Ed. NATURE.]

### Vertebrate Segmentation.

MR. H. G. WELLS, in a recent number of NATURE, honours my little book by making it an example of a contravention of what he regards as a principle of education. With that I have no quarrel. But I must object to the instance he has chosen. The sentences from which he quotes refer to the phenomena of segmentation common to cœlomate tissues, and not to the derivation of vertebrates from any invertebrate group. So far from giving "the impression almost in so many words—'cut and dried,' and ready to be cast into the oven—that the vertebrate type is merely a concentrated derivative (concertina fashion) of the chætopod type," I devote the chapter (xv.) from which he has taken his quotations, to showing that the earthworm and the vertebrates merely belong to two out of the many isolated groups; and at the end of the chapter (though not in spaced type, as I did not consider the question of vertebrate descent congruous with the aims of an elementary text-book) I state that "the type common to the lowest members of the groups of which the earthworm on the one hand, and the vertebrates on the other, form the highest examples, is a simple unsegmented cœlomate animal."

P. CHALMERS MITCHELL.

### The Black-veined White Butterfly.

MR. KIRBY, on p. 340 of your last issue, says (in criticism of Mr. Furneaux) that this insect "would not frequent open ground at a distance from trees." I suppose there are not now many Englishmen who have taken it in this country; and it may be worth while to record that the common on which my brother and I used to find it tolerably abundant in the years 1857–1859, was quite an open place, with no adjacent wood, and very little hedge timber. This common is about a mile and a half to the west of Cardiff; I passed it in the train a few weeks ago, and noted that it is being encroached on by suburbs. We had many a hot chase there over gorse and briar, and always considered this butterfly the most difficult of all to catch. I have never seen it in England since 1859 or 1860.

Oxford, February 11.

W. WARDE FOWLER.

### Parrots in the Philippine Islands.

PRAY allow me space to acknowledge a bad mistake which I first made in the ninth edition of the "Encyclopædia Britannica" (xviii. p. 322), and have lately repeated in the "Dictionary of Birds" (p. 687), by asserting that parrots are "wanting in the Philippine Islands." Seeing that the article was written more than ten years ago, it is quite out of my power to account for the misstatement: my only wonder is that it has not been before challenged, since there is, and has been for some centuries, abundance of evidence to show that there are plenty of parrots in that group of islands, which, indeed, is as well furnished with them (as remarked by my friend Mr. L. W. Wiglesworth, who has kindly drawn my attention to my error) as is the island of Celebes, and I had already (p. 93) noticed the Philippine species of Cockatoo.

Cambridge, February 9.

ALFRED NEWTON.

## *TWENTY-FIVE YEARS OF GEOLOGICAL PROGRESS IN BRITAIN.*

LOOKING back across the fourth part of a century in the progress of any branch of science, we naturally turn first to the list of names of those to whose labours that progress has been due, and though many of these names may happily still be counted among the living, we note many a blank where the hand of death has thinned the ranks. Perhaps in this country no department of natural knowledge can boast a more illustrious bead-roll than that of Geology. The story of the earth had hardly begun to be scientifically studied until the first decades of the present century, and some of the early fathers of geology lived on until well within the life-time of the present generation. A curious transition has thus been going on during the last five-and-twenty years. On the one hand, there have been moving amongst us geological magnates who achieved their fame in the old days when it was still possible for a man to possess a tolerably full personal knowledge of almost every department of the science. On the other hand, around these few living memorials of the heroic age, grew up hosts of younger men, who, finding the main lines already traced for them, have become in large measure specialists, devoting themselves with enthusiasm, but with more restricted vision, to one formation, or one group of rocks, or one tribe of fossils. The days of broad outlines and rapid generalisation have gone. No new systems remain to be added to the geological record of these islands. No new assemblages of extinct types of life now reward the sedulous collector. We have entered upon the era of minute detail and patient elaboration. The field-glass has given way to the microscope. The advance of the science must now be based on laborious research, less brilliant no doubt in its immediate effect, but probably not less lasting in its influence and its results.

Among the great leaders who have passed away within the last twenty-five years are some who have largely helped to mould the whole fabric of geological science. In the philosophy of geology, when will men cease to venerate the names of Lyell and Darwin? In laying down the broad lines of stratigraphy, Sedgwick and Murchison, Phillips, Griffith, Logan, Ramsay and Jukes have left behind them imperishable monuments of their genius. In the palæontological domain, among many other illustrious men, Owen, Lonsdale, Salter, Davidson, Morris, Wright and Egerton have left us. In other departments of the science, our losses have been likewise heavy—the gentle Scrope, pioneer of volcanic geology; Robert Chambers, who, after Agassiz, led the way here in the study of ancient glaciers; David Forbes, who did so much to revive the study of rocks in Britain; as well as men like Page and Ansted, who by their popular writings helped to spread abroad an interest in geology.

Passing from the workers to the work accomplished, we may note a few of the more prominent features in the progress of geology in Britain during the last quarter of a century. Space will not permit the survey to be extended to the history of the science on the continent of Europe and in North America. And first as to the general recognition of the science as an important department of a liberal education. No previous generation has seen so many proofs of this recognition. Many new chairs of Geology have been founded in our universities and colleges. Text-books, class-books, hand-books, manuals and primers of the science have been issued in edition after edition, and new publications are constantly appearing. Field-clubs, and other local associations, have started abundantly into existence, and field-geology is one of their most attractive features. At no time of its comparatively short history has geology been more popular, in the best sense of the word, than it is at the present time.

If one were asked to specify the feature which above all others has marked the progress of geology in Britain during the last five-and-twenty years, one would reply with little hesitation—the enlarged attention given to the study of rocks, or what is termed the petrographical department of the science. For many years in this country that study was almost entirely neglected. The attractions of fossils and of stratigraphy drove minerals and rocks out of the field. As David Forbes used sarcastically to complain, geologists had forgotten that their father was a mineralogist. They allowed the petrography of the British Isles to lapse into a condition of dire confusion, without system, without accurate determinations, and without reference to what had been done in the subject abroad. The first important step in the way of reform was taken by one who is happily still among us, Mr. H. C. Sorby. Reviving the method of making thin slices for microscopical examination, devised by William Nicol, of Edinburgh, he applied it to the study of rocks, and showed how fruitful it might be made in investigating their history. Though his first paper appeared in 1856, it was long in awakening geologists in this country to the value of the new implement of research thus placed in their hands. It attracted notice sooner in Germany, and its applicability as demonstrated there, led ultimately to its adoption in the land of its birth. But only within the last twenty years has it been acknowledged to be absolutely indispensable in the investigation of the origin and history of rocks.

The introduction of the microscope as an adjunct in research has entirely revolutionised the study of petrography. And nowhere has the change been so marked as in Britain. The former chaos has been in large measure reduced to order. The rocks of this country, instead of being neglected, are a foremost object of study, and this branch of British geology has been brought abreast of the petrography of the continent.

It is perhaps inevitable that in such a complete transformation of methods, the new should be apt to be regarded as completely replacing the old way. The microscope has done so much, that its potency may not unnaturally be exaggerated, and a tendency so to magnify it may sometimes be observed. But, after all, the great field-relations of the rocks must in the first place claim our attention and guide our reasoning. The minute structures revealed by the microscope may be made admirably serviceable in controlling that reasoning, and in supplementing the field-evidence by a new body of data otherwise unattainable. Yet the microscope must remain the servant, not the master, in the applications of petrography to the larger questions of geological theory.

If now we turn to the stratigraphical domain of geology, perhaps the first remark that will occur to a reflective observer is that a much closer attention than ever before has been given in Britain to the investigation of the most ancient accessible parts of the earth's crust. The fundamental platform on which the oldest fossiliferous rocks repose, has been searched for with enthusiasm, and though this enthusiasm has led to mistakes, it has undoubtedly been successful in detecting that platform in several places where it was not before supposed to exist. The rocks of the platform have been laboriously investigated, and have been found to include both aqueous and igneous materials. Not only so, but a succession has been observed among them, vast sedimentary masses lying ununiformably on still more ancient gneisses. In these sedimentary accumulations no certain trace of organic forms has yet been detected. Nevertheless the search has not been abandoned. If it should eventually be successful, it would reveal evidence of a fauna or flora older than the oldest relic of life yet discovered in Britain.

In the region where the most ancient gneisses are typically developed, foliated representatives of almost all the well-known plutonic rocks have been recognised, and perhaps also, though dimly, traces of a group of primæval sediments, into which igneous masses have made their way. We have thus been able to take several distinct steps backward into the abyss of time. We know more clearly than before the general outlines of two or more great geological periods anterior to the earliest relics of animal life. And as a band of zealous investigators is busy in the exploration of these dim records, it is perhaps not too much to anticipate a rich harvest of discovery from their labours.

Among the applications of palæontology to the stratigraphical side of geology, unquestionably the most important in recent times has been the recognition of life-zones among the stratified formations, and the adoption of these as a clue to the interpretation of the sequence of strata, and even of tectonic structure. It is long since the ammonite zones of the Lias, first worked out in Germany, were traced in this country. Subsequently the palæontological platforms in the Chalk, so well developed in France, were found to hold good also in England. Still more recently the vertical distribution of graptolites has been shown by Prof. Lapworth to be so restricted that these organisms may be used to mark definite zones in the Silurian system. Nor is it in the animal kingdom only that such restriction has been asserted. The members of an extinct flora have been found to show a more or less marked sequence of genera and species, so that, alike in France and in England, the Carboniferous system has been subdivided into more or less distinct plant-zones.

The value of this palæontological aid in the investigation of stratigraphical succession can hardly be overestimated. Among the undisturbed Secondary rocks of England, indeed, it is not indispensable, for the sequence of their formations and their subdivisions can be ac-

curately determined there from other evidence. Nevertheless stratigraphical arrangement gains much in precision, as well as in scientific interest, when changes in lithological characters are found to be accompanied by changes in organic forms; or, on the other hand, when the succession of animal or vegetable types is found to be repeated in distant localities irrespective of local variations in lithology. But where the rocks have been so folded and broken that from mere mineral characters their true order cannot be made out, the presence in them of determinable life-zones, elsewhere well established, may enable their complicated structure to be unravelled. How this task can be successfully accomplished, has been well shown by Messrs. Lapworth, Peach, and Horne, in regard to the excessively convoluted structure of the Silurian uplands of the South of Scotland.

There is, however, some risk of error in the application of this valuable aid in tectonic investigation. Obviously the existence of a life-zone, which will be of general utility, must be determined upon a basis of evidence sufficiently wide to eliminate mere local peculiarities. It should rest not on the presence of a single species, but on a group of species or genera, for the narrower its palæontological range the greater will be the risk of elevating accidental into general characters. We cannot suppose that a given species began and ended everywhere at the same time or on the same platform. In some areas the conditions would be favourable for its earlier appearance or longer continuance, so that we may expect the zones not to be very sharply defined, but to blend into each other, and in such a way that if we were to define them by single species we should find them to present exceedingly variable limits. In a restricted region, where the sequence of life-zones has been accurately ascertained, these platforms are of great value in working out questions of geological structure. But as we recede from that region the necessity of caution increases. The broad features of biological sequence will no doubt remain, and we shall be able to say where the upper or the lower members of a sedimentary series lie, but we may be led into mistakes by trying too rigidly to make the palæontological zones of one country agree with those of another.

In the department of geotectonics, one of the most interesting features has been the increased attention bestowed upon the nature and results of the great movements that have affected the crust of the earth. The early experiments of Hall, showing that the stratified rocks have undergone enormous lateral compression, have been repeated and extended, and many of the remarkable structures of mountain-ranges have been successfully imitated. More detailed investigation has been bestowed upon plicated and disrupted rocks, especially in Switzerland, Saxony and Scotland. The effects of mechanical deformation in producing foliated structures, even in what were originally massive rocks, have been copiously illustrated. The study of these questions has led to a better appreciation of the enormous plications, inversions, and dislocations which mountain-chains, modern as well as ancient, have undergone. In the Alps and in the Scottish Highlands, the subject has been pursued with great ardour, and these regions will henceforth be classical examples of some of the great features of geotectonic geology.

Another distinguishing characteristic of the last quarter of a century of geological progress has been the increased interest taken in the history of the earth's surface. It is strange that while, generation after generation, men laboured zealously to investigate the history of the planet as recorded in the rocks of the terrestrial crust, they neglected to take account of the superficial topography. They did not realise that every land-surface is a kind of palimpsest, on which the chronicles of a long series of ages may be more or less distinctly traced, and thus that every landscape has, as it were, two histories: first, that of the rocks which form its framework, and, secondly, that of the configuration into which these rocks have been carved. It was in Britain that this fascinating branch of geological inquiry first took definite form in the early days of Hutton and Playfair, and it is here that, after long neglect, it has within the last twenty or thirty years been renewed and pursued with most success. The varied geological structure of these islands, their changeable climate, their mountainous groups, and long lines of sea-beaten coast, make them exceptionally suitable for the prosecution of this inquiry. But this branch of geology is now receiving even more attention in the United States than among ourselves, and in many respects the geological structure of North America offers peculiar advantages for its cultivation.

It is impossible within the limits of this article to do more than present in brief outline a retrospect of a few of the departments of so wide a science as geology. Let me, in conclusion, make reference to but one more subject which has greatly exercised the minds of geologists during the last quarter of a century. It is more than thirty years since Lord Kelvin pointed out that there must be an ascertainable limit to the antiquity of the earth, and that from the data at that time available the limit could not be fixed at less than twenty, or more than 400, millions of years ago. He based this calculation on the thermal conductivity of the globe. Afterwards returning to the subject, he placed the limit within 100 millions of years; and still more recently, reviewing the question in the light of the arguments from tidal retardation and the age of the sun's heat, he has brought down the period of the earth's antiquity to about twenty millions of years.

Geologists have not been slow to admit that they were in error in assuming that they had an eternity of past time for the evolution of the earth's history. They have frankly acknowledged the validity of the physical arguments which go to place more or less definite limits to the antiquity of the earth. They were, on the whole, disposed to acquiesce in the allowance of 100 millions of years granted to them by Lord Kelvin, for the transaction of the whole of the long cycles of geological history. But the physicists have been insatiable and inexorable. As remorseless as Lear's daughters, they have cut down their grant of years by successive slices, until some of them have brought the number to something less than ten millions.

In vain have the geologists protested that there must somewhere be a flaw in a line of argument which tends to results so entirely at variance with the strong evidence for a higher antiquity, furnished not only by the geological record, but by the existing races of plants and animals. They have insisted that this evidence is not mere theory or imagination, but is drawn from a multitude of facts which become hopelessly unintelligible unless sufficient time is admitted for the evolution of geological history. They have not been able to disprove the arguments of the physicists, but they have contended that the physicists have simply ignored the geological arguments as of no account in the discussion.

So here the matter has rested for some years, neither side giving way, and with no prospect of agreement. Within the last few weeks, however, as readers of NATURE will have observed, the question has been taken up anew from the physical side.[1] Prof. Perry, feeling that, after all, the united testimony of geologists and biologists was so decidedly against the latest reductions of time, that it was desirable to reconsider the physical arguments, has gone over them once more. He now finds that on the assumption that the earth is not homogeneous, as postulated by Lord Kelvin, but possesses a much

[1] NATURE, January 3 and February 7, 1895, pp. 224-341.

higher conductivity and thermal capacity in its interior than in its crust, its age may be enormously greater than previous calculations have allowed.

The question being *sub judice*, we must wait until it is settled. But there seems at present every prospect that the physicists will concede not merely the 100 millions of years with which the geologists would be quite content, but a very much greater extent of time.

ARCH. GEIKIE.

## *NOTES.*

THE second of the special meetings of the Royal Society is announced for the 28th inst., when Prof. Weldon will bring forward as a subject for discussion, "Variation in Animals and Plants."

A SUM of 12,000 francs (£480) was voted to the Mont Blanc Observatory by the French Chamber on Tuesday.

PROF. HENRY A. ROWLAND has recently been elected a Foreign Member of the Reale Accademia dei Lincei of Rome, in the section of Physics.

THE death is announced of the Marquis de Saporta, the eminent botanist, at Aix. He was a Correspondent of the Section of Botany of the Paris Academy of Sciences.

MR. REGINALD STUART POOLE, late Keeper of Coins at the British Museum, died on Friday last, in his sixty-third year. Few men have done so much as he to extend the study of Egyptology and antiquities, or have added more to these branches of knowledge. Before he was seventeen years of age he wrote a series of articles on Egyptian chronology, which afterwards appeared in book form under the title "Horæ Ægyptiacæ." He began very early to lecture on Egyptology and numismatics, and in May 1864 made his first appearance in a Friday evening lecture at the Royal Institution. In 1877 he became Keeper of Coins, and during his twenty-two years' tenure of that post he saw through the press thirty-five most valuable catalogues of the collections under his charge. He was the author of the "Cities of Egypt," and of the articles on "Egypt," "Hieroglyphics," and "Numismatics," in the "Encyclopædia Britannica." With Miss Amelia B. Edwards, he was one of the founders of the Egypt Exploration Fund, of which he remained the honorary secretary to his death.

A NEW scientific society, composed chiefly of the professors and assistants in the Paris Natural History Museum, has just been founded. The society owes its existence to Prof. Milne-Edwards, the eminent director of the museum. It is proposed to hold monthly meetings, and to issue a *Bulletin des Naturalistes*, dealing with natural history matters.

THE Manchester Literary and Philosophical Society is fortunate in having Mr. Henry Wilde, F.R.S., for its president. Always one of the best of provincial societies, its usefulness is likely to increase, for Mr. Wilde has intimated his intention to endow the Society with the sum of eight thousand pounds, the annual income from which is to be devoted to various purposes in connection with its work.

THE history of the Museum of the Corporation of London, in the Guildhall, has never been written, although the collections housed therein are of very considerable interest and value. Taking occasion of the visit of the Essex Field Club on Saturday next, Mr. C. Welch, the Curator, will read a paper on the "Origin and Progress of the Guildhall Museum," to remedy this defect. The museum deserves to be better known and better housed than it is at present. A museum fully illustrating the ancient history of London might easily arise from the present collections, and would be a worthy object for achievement for the richest Corporation in the world.

THE Société Technique de l'Industrie du Gaz en France offers several prizes in connection with the Congress to be held during the present year. The *Journal* of the Society of Arts says that the prizes open to all include one of 10,000 francs (£400) offered to the inventor of an incandescent gas-burner showing marked superiority, to be handed in to the Society before April 1 in the present year, unless the committee exercise their power of extending the period for another year. The sum of 8000 francs (£320) will be devoted to various prizes to be awarded to the authors of the best papers on some subject connected with the gas industry, such as the mechanical *manutention* (handling) of coals, cokes, and the various substances used in gasworks, a study of water-gas, and the substitution of hydro-carbons for cannel coal. The papers must be written in French, and not bear the name of the author; but they must contain at the commencement a motto, which must be reproduced on a sealed envelope containing a declaration, signed by the author, that his work is unpublished, and that he will not make any other publication on the same subject within a year. The manuscripts, with sealed envelope, must be sent to the Society, 65 Rue de Provence, Paris, at least forty days before the period fixed for the Congress.

DURING the past week the severe frost has continued over the whole of these islands, and heavy falls of snow have occurred in all three countries. The distribution of pressure has been generally anticyclonic, biting easterly winds, and gales on our coasts. The following are a few of the lowest shade minima published in the *Daily Weather Report* of the Meteorological Office, since we last went to press:—

| February | 7 | 8 | 9 | 10 | 11 | 12 | 13 |
|---|---|---|---|---|---|---|---|
| Nairn | 0° | 6° | 5° | 4° | 3° | 15° | 5° |
| Loughborough | 2 | −5 | −4 | −1 | 1 | 4 | 2 |
| London | 12 | 10 | 11 | 13 | 20 | 14 | 15 |
| Cambridge | 6 | 6 | 7 | 7 | 13 | 15 | 15 |

In the neighbourhood of the metropolis some very low readings were recorded on the 8th instant: Wallington 2°·1, Croydon 5°·5, Tulse Hill 6°, Greenwich 6°·9; and in other parts of the country the following minima have been observed: −12° at Braemar, −8° at Stamford on the 8th, −5° at Glenlee on the 9th, and a still lower reading, viz. −17°, is said to have been observed at Braemar on the 11th instant. The reading of 6°·9 at Greenwich is the lowest but one in the last 50 years, a temperature of 6°·6 having been recorded on January 5, 1860. There had been no reading there lower than 10° in February during the same period, until the present frost, in which lower temperatures have occurred on two successive nights. In London such low temperatures rarely occur; a minimum of −5° was observed by Luke Howard on February 9, 1816. Another feature of the present frost has been the low daily maxima. On the 9th instant it did not exceed 20° at Tulse Hill. At Greenwich the maximum temperature on January 5, 1894, was 19°, which was then the lowest maximum observed there since 1841.

IN a paper read before the British Medical Association in 1889, Dr. A. C. Miller pointed out that, under certain circumstances, advantage might be derived from high level residence in the treatment of tuberculous conditions. A note, which has a bearing upon this view, is contributed by him to the *British Medical Journal*. The observers at the meteorological station on the summit of Ben Nevis are changed every three months or so. While on duty at the observatory, they are, as a matter

of course, exposed to the rigours and inclemencies of the climate on the top of the mountain, and are thus subject to conditions that, *prima facie*, might be expected to favour the occurrence of catarrhs and inflammations. But experience shows quite a different result. The observatory staff while in actual residence on the summit has been remarkably free from all kinds of ailments during the eleven years that have elapsed since the opening. The subsequent residence at low level, however, renders the members of the staff peculiarly liable to an affection which closely resembles, in most of its features, what is usually recognised as an influenzal catarrh. There seems little room for doubt that this condition is due to germ influences in the lower atmosphere. At the summit of the mountain organisms do not exist, or if they do exist, only in innocuous numbers. At the low level they relatively thrive, and, seizing upon the "virgin soil" of a renewed and susceptible mucous surface, they set up the phenomena described.

IN the January and February numbers of the *Geological Magazine*, Mr. E. P. Culverwell criticises severely the astronomical theory of the Ice Age. He concludes that the change in the distribution of the solar heat received by the earth during a period of maximum eccentricity, would not cause a shifting of the isotherms as great as the displacements to which the course of existing isotherms is subject from purely geographical causes. If this is correct, the eccentricity theory of the Ice Age must be abandoned. As a possible alternative theory, Mr. Culverwell suggests a variation in atmospheric pressure through interchange of gases during the earth's passage through space.

SOME interesting additions to Palæobotany are being made by Japanese geologists. In a recent paper (*Journ. Coll. Sci., Imp. Univ., Japan*, vol. vii. part iii. 1894), Prof. Yokoyama gives the results of his study of the fossil plants from several localities on the outer or convex side of that remarkable island. Unlike the plant-bearing beds of the inner, concave side, which, so far as is known, are all Middle Jurassic, the strata from which the flora in question comes appear to be comparable in age to the Wealden of our own country. In the same number, Mr. Nishiwada records the occurrence in Japan of a Tertiary (probably Miocene) limestone with abundant remains of the peculiar calcareous alga or "nullipore," *Lithothamnion*.

PROF. A. ISSEL and Dr. G. Agamennone, who were deputed by the Italian Government to investigate the Zante earthquakes of 1893, have recently issued a valuable report of more than two hundred quarto pages (*Ann. dell' Uff. Cent. di Met. e Geod.* vol. xv. part i. : Rome, 1894). Dr. Agamennone contributes a catalogue of the earthquakes felt in Zante from the earliest times until 1893, and some general remarks on the same, as well as chapters on the relation between the shocks of 1893 and the contemporaneous geodynamic phenomena in Italy, and on the velocity of propagation of the principal shocks. Prof. Issel describes the geology of the island, relates in some detail the features of the recent earthquakes, and concludes the work with a chapter of theoretical considerations.

IN a former note (p. 232), a brief description was given of a remarkable group of earthquake-pulsations recorded at several Italian observatories, and at Nicolaiew on October 27, 1894. It was supposed that these might have proceeded from the great earthquake which occurred on the same day in Chili and the Argentine Republic. The doubt which formerly existed about the time of the shock is now removed by the receipt of detailed accounts. The first pulsations were registered at Rome at 9h. 7m. 35s. p.m., Greenwich mean time, while the shock was recorded at the observatory of Santiago (Chili) at 8h. 50m. 26s., *i.e.* 17m. 9s. earlier. There can thus be little doubt as to the origin of the pulsations, which probably travelled with a velocity of nearly 11 k.m. per second.

A SIMPLIFIED process for silvering glass is described by MM. Auguste and Louis Lumière in the *Journal de Physique*. Take 100 parts by volume of a 10 per cent. solution of nitrate of silver, and add, drop by drop, a quantity of ammonia, just sufficient to dissolve the precipitate formed, avoiding any excess of ammonia. Make up the volume of the solution to ten times the amount by adding distilled water. The reducing solution used is the formaldehyde of commerce. The 40 per cent. solution is diluted to a 1 per cent. solution. The glass to be silvered is polished with chamois leather, and the bath is made up immediately before use, by mixing two parts by volume of the silver solution with one of formaldehyde. The solution must be poured right over the surface without stopping. After about five or ten minutes, at a temperature between 15° and 19° C., all the silver in the solution is deposited on the glass in a bright layer, which is then washed in running water. It is then varnished if the glass side is to be used, or polished if the free surface is required for reflection. This method does not require the scrupulous care necessary with other methods, but Brashear's process, for instance, gives mirrors which do not require polishing.

THE Government of Cape Colony has recently instituted inquiries in various quarters with a view to gain suggestions as to the best methods of developing the neglected sea-fisheries of the colony. The Minister of Agriculture has received a number of replies upon the subject, which have been published in the *Agricultural Journal* of Cape Colony. The first desideratum in a matter of this important nature is clearly that the Government should possess some trustworthy and responsible source of information upon matters connected with its sea-fisheries; and on this account Dr. Trimen, the Curator of the South African Museum, strongly recommends the Government to obtain the services of a competent naturalist, accustomed to the study of marine animals and, if possible, experienced in the work of English or American Fishery Departments, who would devote himself to a thorough study of South African Fisheries. The appointment of skilled naturalists as Fishery Advisers to Governments has been attended with excellent results in Holland and other countries, and we heartily endorse all that Dr. Trimen has to say in favour of the adoption of a similar course in the present instance.

MESSRS. LAWS AND ANDREWES have not finished their vindication of the innocuous nature of sewer air. We thought that from the microbic point of view this question had been disposed of; but the London County Council evidently think differently, and another report is to hand, telling us that the microbes present in sewage are not present in sewer air, and that there is no microbic relationship, either in quantity or quality, between the two. These investigators claim to have found a bacillus resembling the typhoid bacillus in a drain from the typhoid block of a fever hospital in which no disinfection had been carried out for two days previously. This is hardly "an important fact," and perhaps not an unexpected discovery; but it is interesting to note that when typhoid germs are placed in sterile sewage, they are said to succumb rapidly, whilst the closely allied *B. coli communis* multiplies extensively. Details are given as to how the sewage was sterilised; but whether culture material was introduced along with the respective organisms, is not mentioned. We fail to understand why the colon bacillus is described as "harmless"; it has been shown to possess very distinct pathogenic properties. Experiments were conducted to ascertain what effect certain sewage microbes would have on the vitality of the typhoid bacillus in

sterile sewage, whether the demise of the latter would be hastened or retarded by their presence. In these investigations we are informed that the respective microbes were inoculated into the sewage *direct from broth cultures*, so that we cannot accept the results obtained as representative. It has been repeatedly shown what an effect the presence of even minute traces of culture material has on the vitality of particular microbes in water, &c., and what confusing and conflicting results have been obtained by different observers working with the same microbe through neglect of this simple precaution. It is to be hoped that if any further experiments in this direction are contemplated, Messrs. Laws and Andrewes will bear this fact in mind.

THE inaugural lecture "On the Present Relations of Agricultural Art and Science," delivered by Prof. R. Warington, F.R.S., in the University Museum, Oxford, on the 4th inst., has been published by Mr. Henry Frowde. By a few statements of fact, the Sibthorpian Professor shows that the agricultural industry can only be placed upon a sound basis by the full adoption of scientific methods and the recognition of research. It is very well known that the provision for agricultural investigations in England is at present wholly inadequate. We have the important experiment station at Rothamsted, and the Royal Agricultural's Society's station at Woburn. Experiments are also carried on at some of our agricultural colleges; but this represents practically all that is being done here for the elucidation of agricultural problems. Compare this with the provision made in other countries. "In the German Empire alone there are fifty-four agricultural experiment stations, besides numerous public laboratories for the analysis of manures, and seed testing. The experiment stations are supported by aid from the State. The whole number of experiment stations on the continent of Europe is about 190. There are besides these, about 120 public laboratories devoted to special kinds of agricultural work. In the United States, a great effort has recently been made to provide the whole country with experiment stations. Every State in the Union has now at least one station, supported, since the year 1887, with an annual income of about £3000 paid to it by the nation. The total number of experiment stations in the United States is at present fifty-five . . . the practical working out of agricultural problems can only be accomplished by the establishment among us of a considerable number of experiment stations; only in this way can science be brought into touch with practices, and the teachings of science reduced to that concrete form in which they can be put to profitable use by the farmer. Why should not our County Councils found and maintain an agricultural experiment station in each county? Such a station would become a centre of light and teaching to a large district."

THE *Proceedings* of the eleventh annual convention of the Association of Official Agricultural Chemists, just distributed by the U.S. Department of Agriculture, enforces the remarks referred to above. The volume is literally full of facts relating to agricultural chemistry. It contains many original articles, abstracts of papers, analyses of fertilisers, foods, soils, and farm products, and numerous reports. Unfortunately, the results of such investigations in the United States are not fully applicable to British agriculture. What is wanted, as Prof. Warington points out, is a central agricultural laboratory in England, having as one of its duties the preparation of chemical agricultural statistics. When this has been established for a few years, agriculturists will not have to confess that they know more about the composition and properties of German and American produce than they do of similar products in our own country.

THE tenth of the Alembic Club Reprints, published by Mr. W. F. Clay, Edinburgh, contains Graham's most valuable contribution to pure chemistry, viz., the paper read before the Royal Society in 1833, entitled "Researches on the Arseniates, Phosphates, and Modifications of Phosphoric Acid."

ALL that is readable about comets is to be found in Mr. Thynne Lynn's "Remarkable Comets" (Edward Stanford), the third edition of which has just been published. Though but a slender brochure of less than fifty pages, this little treatise comprises most of the interesting facts in the history of cometary discovery.

THE new illustrated catalogue of electrical and other apparatus, just published by Messrs Elliott Brothers, is capable of giving the student of elementary science a liberal education in scientific instruments. Almost every important piece of apparatus used in electricity is illustrated in the catalogue, as well as the tools of research and education in other branches of physical science.

THE Calendar of the Department of Science and Art, for 1895, has been issued. For the benefit of those unfamiliar with this publication, it may be worth mention that the Calendar comprises a history and general description of the whole Department, with a summary of the rules, and a list of the Science and Art Schools and Classes, containing the number of students in each school and in each subject.

IN the *Bulletin* of the College of Agriculture of the Imperial University of Japan (vol ii. No. 3), Prof. Sasaki gives a full description of the scale insect of mulberry trees (*Diaspis patelliformis*, n. sp.) and of the principal features of its life-history. A great deal of damage seems to be done by this insect pest; and it would be well if tree-owners in Japan would make use of the simple remedial measures recommended by Prof. Sasaki. The *Bulletin* also includes a paper on the spermatogenesis of the silkworm by K. Toyama.

IN 1881, the first edition of "Elementary Lessons in Electricity and Magnetism" (Macmillan), by Prof. S. P. Thompson, F.R.S., was published. Since then the book has been reprinted, with alterations and additions, many times, and an entirely new edition has now been issued. We are reminded in the preface that during the fourteen years which have elapsed since the book first appeared, electrical science and the science of magnetism have been greatly advanced. Striking progress has been made in theory and in practice, and the expansion of knowledge has necessitated an expansion of the book. Whoso seeks a class-book on electricity and magnetism, containing an elementary exposition of recent work, will find their want supplied by Prof. Thompson's Lessons.

In *Science Progress* for February, Dr. C. S. Sherrington F.R.S., describes the varieties of leucocytes. Mr. A. C. Seward concludes his second contribution on the structure and formation of coal, with a paragraph which expresses his conviction that "the weight of evidence seems to tip the balance of opinion very materially towards the theory of drifting and subaqueous sedimentation, for the majority of the Palæozoic coal seams." An account of the researches on the methods of digestion in Cœlenterates, is given by Prof. S. J. Hickson; Dr. W. F. Hume reviews works on geological folds and faulting; and Mr. Philip Lake adds to the literature of tectonic geology, a paper on Neozoic—that is, Mesozoic and Cainozoic—geology in Europe.

AT present the English Arboricultural Society hardly justifies its name, for it is more or less limited to the North of England. There is every prospect, however, of its extension southwards in the future, both on account of the attention now

being given to forestry, and because the Society is making efforts to increase its membership by going further afield. The *Transactions* of the Society, of which we have received a recent issue (vol. iii part iv.), should materially assist towards this end; for the instructive papers contained therein appeal to all foresters. In the Part received by us, there is an essay on "How Trees Grow," by Mr. J. Maughan; an essay on "Thinning Mixed Plantations," by Mr. W. Forbes; and a paper on "The Distribution of Trees in a Wood," by Prof. W. Somerville. These are all useful contributions, but even more important is the publication of the results of an inquiry, conducted by the Society, into the disease of the Larch, a subject about which much has been written and said. The information which Prof. Somerville has brought together, forms a valuable summary of the present position of knowledge in regard to the Larch disease, and shows the various conditions and cultural methods which hold out some prospect of securing comparative immunity from attack. With a larger membership, the Society would be able to carry out, and publish, the results of many similar investigations.

PROF. SCHIFF and Dr. Tarugi describe, in a communication to the *Berichte*, an admirable substitute for the disagreeable sulphuretted hydrogen in qualitative analysis. The new reagent is the ammonium salt of thio-acetic acid, $CH_3 . COSNH_4$. Ammonium thio-acetate is decomposed by hot dilute hydrochloric acid, liberating sulphuretted hydrogen without any precipitation of sulphur. No objectionable bye-products are formed in the reaction, only sal-ammoniac and acetic acid being produced. When a feebly ammoniacal solution of ammonium thio-acetate is added to a hydrochloric acid solution of the metals of the second group, and the liquid is heated to near boiling, the metals are at once precipitated as sulphides, while only the faintest odour of sulphuretted hydrogen is perceptible. After cooling and filtering, the filtrate is found to contain no trace of the metals, not even of arsenic if an arseniate had been originally present. The completeness and rapidity of the reaction, particularly in the case of arsenic, which is usually so troublesome to precipitate from arseniates, is one of its strongest recommendations, and is described by Prof. Schiff as being perfectly surprising. A couple of cubic centimetres of a 30 per cent. solution of ammonium thio-acetate is usually ample for a gram of the substance to be analysed. The reagent has been employed for some time by Prof. Schiff in the Pisa laboratory, and is much appreciated by his students, sulphuretted hydrogen being completely excluded from the laboratory, doubtless to the material advantage of all concerned, both as regards comfort and health. Thio-acetic acid is readily prepared by acting upon glacial acetic acid with phosphorus pentasulphide. It boils at 95°, and is but slightly soluble in water. When the acid is dissolved in a slight excess of dilute ammonia, a yellow solution is obtained, which is then diluted to three times the volume of the acid originally taken—that is, 10 cubic centimetres of the acid furnish 30 cubic centimetres of the reagent. Prof. Schiff serves the reagent out to his students in small bottles closed by a cork, through which a small pipette, holding 2 cubic centimetres, is inserted, by means of which the convenient quantity of the reagent can at once be added to the hydrochloric solution of their test substance. It is scarcely necessary to add that zinc, manganese, nickel, and cobalt are not precipitated in the presence of hydrochloric acid by the new reagent, any more than by sulphuretted hydrogen itself. The sulphides of these metals are at once precipitated, however, upon rendering the solution alkaline; but as ammonium sulphide acts quite as well for this purpose, Prof. Schiff confines the use of ammonium thio-acetate to the precipitation of the metals of the second group.

THE additions to the Zoological Society's Gardens during the past week include a Lion (*Felis leo*, ♂) from India, presented by H.R.H. the Duke of Connaught; a Cape Bucephalus (*Bucephalus capensis*) from South Africa, presented by Mr. J. E. Matcham.

## OUR ASTRONOMICAL COLUMN.

THE MASS OF THE ASTEROIDS.—A preliminary note on the probable mass of the asteroids was contributed by Mr. B. M. Roszel to the *Johns Hopkins University Circular* in May 1894, and summarised in these columns (NATURE, vol. l. p. 87.) In that paper Mr. Roszel limited himself to determining the mass from a study of 216 minor planets; he has now extended the computations to 311 asteroids, the orbits of which are given in the *Berliner Astronomisches Jahrbuch* for 1894 (*University Circular*, January 1895). His chief object was to find a probable limiting value for the mass, rather than an accurate determination of the mass itself. Using the photometric determinations of the diameter of Vesta, by Pickering and Müller, and the direct measures of the diameters of Ceres, Pallas, and Vesta by Barnard, Mr. Roszel has reduced the volumes of all the asteroids to the volume of Vesta, except when Barnard's measures were the basis, in which case he computed the volumes of Ceres and Pallas separately, and added them to the combined volume of the remaining 309. Assuming the albedo constant and a constant density equal to the density of Mars, he obtained the following numbers:

| | |
|---|---|
| Combined volume of 311 asteroids . | 5·185 vol. of Vesta. |
| ,, ,, omitting Ceres and Pallas. . . | 2·684 ,, ,, |
| Volume of Vesta, in terms of volume of Mars: | |
| (1) From Pickering's estimate of the diameter of Vesta, assuming the albedo = albedo of Mars . . . . . . . . | = 0·00022 |
| (2) From Müller's estimate, assuming (*a*) the albedo = albedo of Mars . . . | = 0·00065 |
| (3) From Müller's estimate, assuming (*b*) the albedo = albedo of Mercury . . . | = 0·00129 |
| (4) From Barnard's estimate of the diameter of Vesta . . . . . . . | = 0·00018 |
| Combined mass of 311 asteroids, in terms of mass of Mars: | |
| From Pickering's estimate, as above . . | = 0·00112 |
| ,, Müller's estimate, as above (*a*) . . | = 0·00336 |
| ,, ,, ,, ,, (*b*) . . | = 0·00666 |
| ,, Barnard's measures . . . . | = 0·00273 |

The mean diameter of Mars was taken as 4230 miles. It appears from the above numbers that the combined mass of the asteriods is about ·026 of the mass of the moon.

THE APPARENT DIAMETERS OF MERCURY.—During the transits of Mercury on May 9, 1891, and November 10, 1894, Prof. Barnard gave special attention to measurements of the planet's apparent diameter. (*Ast. Jour.* No. 335.) In both cases the diameter in R.A. was found to be slightly greater than that in declination, and if this be not a mere accidental coincidence, as Prof. Barnard seems rather inclined to believe, it would indicate a small polar compression. The measures at the two transits respectively indicate a polar compression of 1/134 and 1/98, or a mean of 1/116. Though by no means insisting on the reality of the difference in the diameters, Prof. Barnard points out that "the results may be of great importance in the future, as bearing upon the rotation of the planet on its axis in a reasonably short period." Expressed in angular measure, reduced to unit distance, the two diameters as measured in 1894 were 6"·241 for the "equatorial," and 6"·178 for the "polar" diameter. It is incidentally mentioned that "though Mercury cannot be seen at transits with the naked eye alone, it only requires a power of 2½ diameters to make it easily visible."

THE VARIATION OF LATITUDE.—The results obtained by Mr. Chandler in his investigations of variation of latitude seem to be confirmed by M. Ivanof's recent discussion of the older series of observations made with the great vertical circle at Pulkowa. (*Ast. Jour.* No. 335). "There is no doubt that two periods subsist; one equal to 428 days, the other to a year. Also, the semi-amplitudes of both terms are variable beyond any doubt."

## THE SUN'S PLACE IN NATURE.[1]

### I.

WHEN, in 1886, it became my duty to give a course of lectures here, I thought it advisable to deal with the sun and stars, not with reference specially to solar physics, but in order to give a general idea of two important lines of work which were running then nearly parallel to each other, and promised soon to meet, with the greatest benefit to science. Only a very little was said in those lectures touching the relation of stars to nebulæ, and the various views which have been held time out of mind with regard to the special nature of both these classes of celestial bodies. Such questions, however, have always had the greatest interest for mankind, for those at all events among us who like to know something about the universe in which our lot is cast. No dividends, unfortunately or fortunately, depend upon the discussion or even the application of any branches of inquiry which are necessary in order to make progress along the lines of thought thus opened up; scant attention is paid to them by educational bodies, for they lead to no profession; but in spite of that, some of the noblest triumphs of the human mind have been made in that region where man finds himself face to face with the mysteries of the distant heavens.

To consider completely the Sun's Place in Nature, which is the subject I have chosen for this present course of lectures, the relation of these two apparently different classes of celestial bodies to which I have referred, must be gone into. Thanks to the advance of modern science, I shall be able by-and-by to throw upon the screen pictures of clusters of stars, and of nebulæ, in which you will see those bodies very much better than you could do if to-night you were in one of the best equipped observatories in the world, for it so happens that the enormous progress which has recently been made in the application of photography to astronomical work enables us to get permanent records of parts of them which are so dim that they never have been and never will be directly revealed to the eye of mortals.

When we compare these two great groups of celestial bodies we find that, at all events in appearance, there is an enormous difference between them; that a nebula is certainly unlike a star, or even an ordinary star cluster. This is so obvious that even those who first observed those very few nebulæ which are visible to the naked eye (such a one, for instance, as that which is now beautifully visible to us in the early night in the nebula of Orion, or the other in Andromeda, which we can see almost throughout the year), the greatest wonderment was caused by their strange appearance.

Let us go back 150 years. I have here a book ("Les Hypothèses Cosmogoniques"), recently written by a distinguished French astronomer, M. Wolf, which contains a reference to what the French philosopher Maupertuis said about them in the year 1745. "The first phenomenon is that of those brilliant patches in the sky which are named nebulæ, and have been considered as masses or groups of small stars; but our astronomers, with the aid of better telescopes, have only seen them as great oval areas, luminous and with a light brighter than the rest of the heavens. Huygens first discovered one in the constellation of Orion; Halley, in the *Philosophical Transactions*, pointed out six, the first in the Sword of Orion, the second in the constellation of Sagittarius, the third in the Centaur, the fourth before the right foot of Antinoüs, the fifth in Herculis, and the sixth in Andromeda. Five of these spots having been observed with a reflector of 8 ft., only one of them, the fourth, could be taken for a group of stars; the others seem to be great shining areas, and do not differ among themselves, except that some are more round and others more oval in shape. It seems also that in the first the little stars which one discovers with the telescope are not capable of causing this brightness. Halley was much struck with these phenomena, which he believes capable of explaining a thing which seemed difficult to understand in the Book of Genesis, viz. that light was created before the sun. Durham regards them as holes through which one discovers an immense region of light, and finally the empyrean heaven itself. He professes to have been able to distinguish that the stars which are seen in some of them are very much less distant from us than the spots of light themselves."[2]

I need not follow the quotation any further, but you see that 150 years ago some of our keenest intellects were struggling with the questions involved in mystery which had been started by the discovery of these nebulous bodies in space. That was in the year 1745. Soon after this, in the year 1755, Kant, who was a German, though he was by direct descent a Scotchman, brought out an hypothesis in which he attempted to show that there was the closest possible connection between stars and the clusters and nebulæ of which Maupertuis spoke. He held distinctly that the stars were produced by some action brought about in nebulæ; in other words, that the nebulæ represented a first stage out of which stars, representing a later stage, were produced by certain processes of evolution.

From 1755 we pass to 1796, at which date we find a great Frenchman (Laplace) practically rediscovering and reasserting the same thing. It is believed that he knew nothing of Kant's prior work, and therefore we have the advantage of dealing with the results of the thoughts of two great minds. Laplace came to the same conclusion as Kant, so far as it went, but he went further than Kant did, because he held that the nebulæ really represent enormous masses of elastic gas at high temperature, and that therefore the stars, which he conceived, as Kant had conceived, to be produced by evolutionary processes from these nebulæ, were really produced from incandescent masses of gas.

Now, seeing that our sun is a star, it is perfectly clear from this that both Kant and Laplace agreed that the sun, representing a star, had originally been produced from a nebula. That is my first point.

About the time of Laplace, *i.e.* about 1796, Sir William Herschel was making England famous by the discoveries rendered possible by that wonderful telescope which he had erected at Slough. There, for the first time, the possible similarities and the possible differences of these two great groups of celestial bodies were subjected to the most minute and laborious scrutiny. Well, he came absolutely to the same conclusion as his predecessors had done, and for Sir William Herschel there was no doubt whatever that from the most irregular nebula to the densest star there was a gradual process of change; that there was no radical difference, but that the star represented simply the result of certain evolutionary changes. This view thus strengthened held the field for some years; then a larger telescope was made by Lord Rosse, a 6-foot mirror was now available instead of the 4-foot one which had been erected by Herschel at Slough. Lord Rosse—you will find the whole story admirably told in Prof. Nichol's book, "The Architecture of the Heavens"—came to the conclusion that when he observed a so-called nebula on the finest possible nights, when the air was stillest, and the magnifying power which he could use was greater than usual, he could see what he called the possibility of a resolvability in it. That is to say, nebulæ might after all really be star clusters, only immensely remote, so that the light of all the stars was, as it were, so welded together as to give that appearance of a candle seen through horn, which Maupertuis and his predecessors had observed.

Next we come to the year 1862, and we find a new instrument brought to bear, which at once drove into thin air all the statements which had been made on what had turned out to be a line of inquiry which was incapable of giving a final verdict. It so happened that in that year there was a very powerful combination formed by a distinguished chemist and philosopher, Dr. William Allen Miller, the Treasurer of the Royal Society, who had already done most admirable spectroscopic work, and a neighbour of his, Mr. Huggins, who had mounted a powerful telescope in 1856. The spectroscope, which was then practically a new instrument, was applied to the telescope.

I need not say much about the spectroscope, as I have already had an opportunity of describing it to some of you, but I may in a few words show exactly the function of this new instrument of enormous power, which has in a very few years perfectly changed the aspect of astronomic science. If we pass a ray of white light through a piece of glass called a prism, we find that after the light has so passed through, it is changed into a beautiful band, showing all the colours of the rainbow. This prism then is the fundamental part of the instrument which is called the spectroscope, and the most complicated spectroscope which we can imagine simply utilises the part which this piece of triangular glass plays in breaking a beam of light of any colour

[1] Revised from shorthand notes of a course of lectures to working men at the Museum of Practical Geology during November and December, 1894.

[2] "Discours sur les différentes figures des Astres," chap. vi. pp. 104-14.

into its constituent parts from the red to the violet; between these colours we get that string of orange yellow, green, and blue which you are familiar with in the rainbow. For sixpence any of you may make for yourselves an instrument which will serve many of the purposes of demonstrating some of the more beautiful fields of knowledge which have been opened up

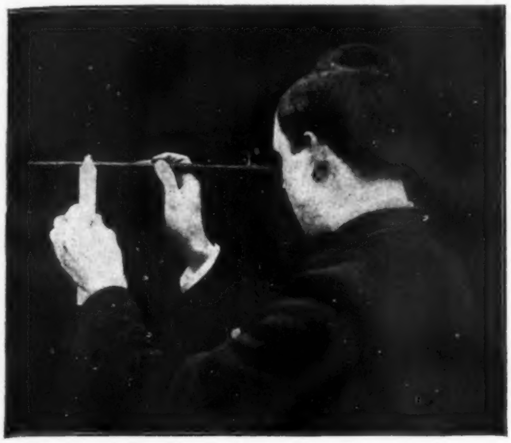


FIG. 1.—A simple form of spectroscope.

to us by its use. From an optician you can get a tiny prism for sixpence; glue it at one end of a piece of wood about $12 \times 1 \times \frac{1}{2}$ inch, so that you can see through it a coloured image of a needle stuck in at the other end of the piece of wood (Fig. 1). This you must do by looking sideways through it. Allow your needle to be illuminated by a candle or a gas flame, taking care that the direct light from the candle does not fall upon the face of the prism; you will then get a complete band of colour from red to blue. If you go into the sunlight—taking care again to protect the prism itself from the entrance of any foreign light—and allow the sunbeam to illuminate your needle, you get a spectrum of a different kind, full of black lines.

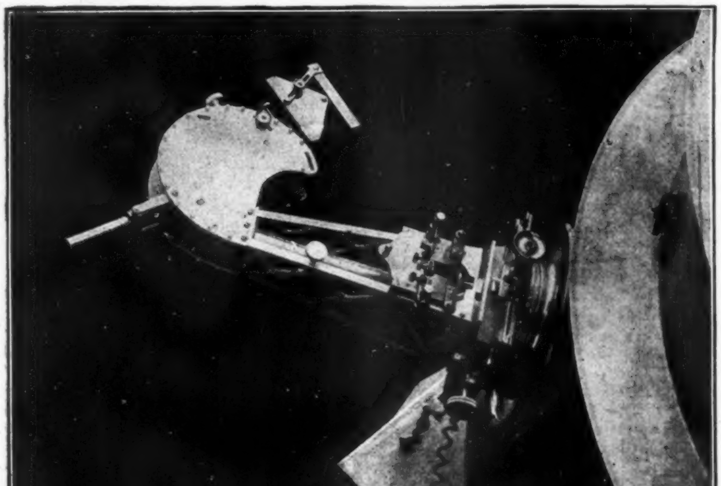


FIG. 2.—Star spectroscope, arranged for photographing, attached to eye-end of reflecting telescope.

By such experiments as that, certain spectroscopic axioms have been formulated: three of them are very important.

First, when solid or liquid or densely gaseous bodies are incandescent, they give out continuous spectra.

Second, when a solid or liquid body reduced to a state of gas, or any gas itself, is giving light, the spectrum consists of bright lines, and these lines are different for different substances.

Third, when light from a solid or liquid body passes through gas at a lower temperature, the gas absorbs those particular rays of light of which its own spectrum consists.

We will next suppose, then, a spectroscope placed at the eye-end of a telescope (Fig. 2). The question put to the combined instruments is: What is starlight like? It was found that the stars give a spectrum very much like the spectrum of the sun, in most cases at all events, and that this spectrum could be defined in the light of the third axiom, that certain of the light was absorbed, there were dark lines in the spectrum (Fig. 3); and thus we knew that light had been absorbed by an atmosphere surrounding something which was very much hotter than itself, and in that way the science of solar and stellar physics was founded.

Suppose another question put to this instrument: What is the light of the nebulæ like?

I have already told you that Laplace held that in these bodies we were dealing with gas at a high temperature. From the time of Tycho Brahe downwards, people had an idea that the nebulæ were "fiery." What should we expect to get in our instrument? The second axiom tells us that, if we are dealing with matter in a state of gas, or anything vapourous at very high temperature, we shall get bright lines only. The question as to the nebulæ was put in 1864, and, curiously enough, when the observation was made, Dr. Huggins remarked: "I suspected some derangement of the instrument had taken place, for no spectrum was seen, but only a line." "Only a line" was exactly what I suppose Laplace would have given all he possessed to see, if spectrum analysis had been invented in his day. That line settled the question. There was certainly a tremendous spectroscopic difference between stars and nebulæ, and this difference has been emphasised by subsequent researches. (See Fig. 3.) It is evident, therefore, that Lord Rosse's suspicion that the nebulæ might, after all, be found to be resolvable into star clusters when greater optical power was used, was proved to be erroneous.

Now we come to the second point. I indicated in the previous course of lectures that there were differences among the stars, depending possibly upon chemical constitution, or temperature, or even upon their ages, and that the stars had been classified by several very diligent inquirers. Also, that in all the classifications that had been attempted, it was universally taken for granted, for some reason or other—possibly in view of the idea of Laplace—that all the stars in the heavens began in the condition of highest temperature, and that all that the stars did after that was to spend their millions and billions of years of life in getting colder; so that, if we could at the present moment find out which was the very hottest star in the heavens, we might be perfectly certain that every star in its beginning resembled exactly in spectrum, and therefore in physical constitution, that particular star which we suppose to be the hottest. It so happened that in that very course of lectures I pointed out, for the first time, I think, in reference to the separation of stars into classes, that such an idea as that would never do; for if we form any conception of nebulæ changing into stars, we begin by knowing that the stars are very much denser than the nebulæ—taking the sun as an instance, the star practically close to us—and that as the stars are denser than the nebulæ, they must be hotter than the nebulæ, instead of being colder.

This depended absolutely upon the application of thermo-dynamics, and had been pointed out by Helmholtz in the year 1845. Sir William Thomson, now Lord Kelvin, also pointed out quite distinctly that the hypothesis of fiery nebulous matter—by that meaning nebulous matter hotter than the stars—was invented before the discovery of thermo-dynamics; otherwise, he said, the nebulæ would certainly never have been conceived to have been fiery, *i.e.* something hotter than the average star.

I then went on to show that Lord Kelvin told us how he could imagine a condition of nebulæ which might ultimately condense into stars without violating the laws of thermo-dynamics, which were completely traversed by Laplace's view; and he referred to a suggestion that had been made by Prof. Tait, who supposed that the luminosity of nebulæ, and even the spectroscopic appearances which have been observed, might be explained by supposing that we were dealing with gaseous exhalations proceeding from the collisions of meteoric stones; and he also pointed out that possibly that would not only explain the luminosity of nebulæ, but the luminosity of comets as well. By the kindness of the Director of this Museum, I have some specimens of these meteoric stones on the table. I would remind you that the few specimens which I have here have been selected from the magnificent collection upstairs; if you have a few minutes to spare after this lecture, you cannot do better than go and have a look at them, and you will see how very various both to the eye and in chemical and physical constitution they are. Let me also recommend you to get a little pamphlet (price 2*d.*) containing a description of the meteorite collection in the Natural History Museum, which is one of the finest in the world.

We thus arrived at the idea that these wonderful nebulæ may be explained, apart from any fiery gas; that we have simply to look to a meteoritic origin to explain both the appearances and the spectrum.

After that point had been made, I went on to make another. I had already referred to the classification of stars, and I remarked that if one looked at the different groups of spectra, it seemed as if a classification of them, based on these ideas, did fit the facts better, the existing ones depending on the unphilosophic one of Laplace. It is possible, I said, that the great differences which had been observed in the spectra of comets and of nebulæ, although the origin of the light of both was ascribed to the clashing together of stones in different parts of space, might be explained by the fact that, in consequence of the collisions between these bodies occurring under different conditions and at different velocities, there would be very considerable differences in the temperatures produced in the two cases. Similar conditions might hold for stars in different degrees of condensation. It was also suggested that this new idea might explain the phenomena of variable and new stars, which have always been accounted to be the most extraordinary and mysterious in the whole domain of astronomy; and, finally, I said the subject was well worthy of study, because it seemed as if many phenomena on the nearest star to us, our own sun, might be really phenomena produced by the fall of meteoric bodies upon that surface which we see, and which we call the photosphere. It is now many years ago since Balfour Stewart and others threw out the idea that the phenomena connected with the formation of sun-spots were really produced by the fall of bodies upon that surface. Other philosophers have preferred the idea that we have to do with eruptions from the interior of the sun; nothing can be more divergent than the opinions which have been brought forward as explanations of these appearances.

But you at once see that, if we assume that this meteoritic action may take place in the solar atmosphere, it need not necessarily be a meteoritic action coming from without. Taking our own case, we live in a damp climate, and sometimes the air is dampest when there are no clouds. Clouds are condensations of the moisture in the air, and we know that it is not really a question of clouds only; we may have snow, rain, or hail, and all these represent different condensations of the damp—or, as we call it, the aqueous—vapour which is ever present in our air. Apply that to the sun. What is the air of the sun composed of? Well, certainly one important constituent of it is the incandescent vapour of iron; we are no longer dealing with a low temperature and the vapour of water, but with an atmosphere in the hotter parts of which iron is not solid or liquid, but in which the temperature is high enough to keep it in a state of gas, probably thousands of degrees higher than is arrived at in the Bessemer process.

We will assume, then, that that temperature and that condition of atmosphere prevails for 20,000 (it is probably nearer 50,000) miles above the photosphere of the sun. As we get further from the sun, the atmosphere is of course getting cooler, and at a certain distance above the photosphere the temperature will be so reduced that the iron vapour might play the part of our aqueous vapour; then it condenses and turns into iron snow

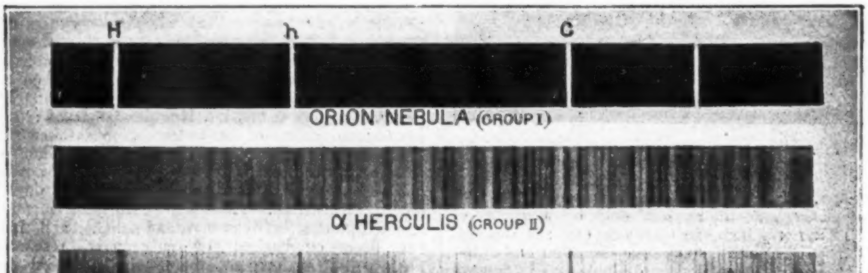



FIG. 3.—The photographed spectrum of a nebula, contrasted with the spectra of stars.

and iron hail and iron rain, and so on, falling upon the photosphere as the rain falls on the earth. There is thus a possibility in the sun of home-made meteoritic action.

So far as my last course of lectures was concerned, I there ended that part of the subject. But so many points had been raised in trying to give a connected view of these two very slowly converging lines of research to which I referred, that, after the lectures were over, I determined to discuss the various points which had been raised. I determined to take up Prof. Tait's suggestion, and see how all the spectroscopic observations which had been made up to the time of my lectures in 1886, bore out that suggestion which had been made in 1871, before there was very much spectroscopic evidence to go upon. The result was that my assistants and myself spent something like three years in gathering together, we believe, every available observation; at all events, if not every available observation, there were between thirty and forty thousand of them, and we found that a very considerable number. I not only determined to collect them, but also to discuss them, and make any experiments or observations which might be suggested by the discussion. The result of this was that, as a fruit of that course of lectures, several papers, some of them very long—it is not for me to say anything as to their value—were sent in to the Royal Society, and eventually brought together in a book.

Now, what I found was that when we discussed the meteoritic view in the light of all the observations we could get together, and in relation to stars as well as nebulæ and comets, it seemed to explain many things, and threw a perfectly new light upon the visible universe; there were, moreover, several points raised of intense novelty and freshness, each of which could be discussed separately, cast aside if it were false, and held on to if it were true. I give a table of some of these new points of view.

*New points of view in the Meteoritic Hypothesis.*

(1) There is the closest possible connection between nebulæ and stars.

(2) The first stage in the development of cosmical bodies is not a mass of hot gas, but a swarm of cold meteorites.

(3) Many bodies in space which look like stars are really centres of nebulæ; that is, of meteoritic swarms.

(4) Stars with bright-line spectra must be associated with nebulæ.

(5) Some of the heavenly bodies are increasing their temperatures; others are decreasing their temperatures.

(6) Double swarms, in any stages of condensation, may give rise to the phenomena of variability.

(7) New stars are produced by the clash of meteor swarms. They are closely related to nebulæ and bright-line stars.

(8) Cosmical space is a meteoritic plenum.

(9) A new classification of the heavenly bodies, based on the varying states of condensation of the meteoritic swarms.

(10) The sun is one of those stars the temperature of which is rapidly decreasing.

(11) Many of the changing phenomena of the sun are due to the fall of meteoritic matter upon the photosphere.

We ultimately arrived at the conclusion that the sun is one of the stars, the temperature of which is gradually decreasing, and that many of the phenomena of the sun are due to the fall of meteoritic matter on the photosphere.

The doing of a large piece of work like that—and I say it is large because I am glad to have the opportunity here of expressing my gratitude to my assistants, who stood by me for three years—brings one out pretty well into the open, and renders one liable to a brisk fire of criticisms, some very valuable, some quite unworthy of the critics.

You will see that the work was undertaken with a view of determining the sun's place among the stars.

J. NORMAN LOCKYER.

(*To be continued.*)

## THE INSTITUTION OF MECHANICAL ENGINEERS.

THE annual general meeting of the Institution of Mechanical Engineers was held on Thursday and Friday evenings, the 31st ult. and the 1st inst. Prof. A. B. W. Kennedy, F.R.S., occupied the chair. There were two papers set down for reading and discussion:

"The Determination of the Dryness of Steam." By Prof. W. Cawthorne Unwin, F.R.S.

"Comparison between Governing by Throttling and by Variable Expansion." By Captain H. Riall Sankey.

Prof. Unwin, in his interesting paper, gave descriptions of the best known methods of determining the mean of moisture in steam up to now introduced. Most of the apparatus described was exhibited on the table of the theatre, whilst diagrams illustrative of them were hung on the wall. The author pointed out that the earliest attempts to determine the amount of moisture in steam, of which records have been found, were made during some boiler trials carried out by a committee of the Société Industrielle of Mulhouse in 1859. This committee tried three different methods—a method of separation, a condensing method suggested by Hirn, and a chemical method. In these early trials the condensing method only, in which the total heat of a sample of the steam was measured, appeared to give satisfactory results. But although the committee did not place full reliance on any of their methods, these have all been used by various experimenters down to the present time.

The origin of water entrained in steam, Prof. Unwin said, was to be attributed to three causes:

(1) Water projected into the boiler's steam space during ebullition. The extent to which wetness occurs depends on the activity of the ebullition, the area of the water surface, the volume of the steam space, the position of the steam valve, the density of the steam, and, probably more than anything else, on the quality of the water and its liability to produce foam. The author referred to the experiments of Mr. Thornycroft, who constructed a boiler with glass ends, through which the process of boiling could be seen. The result of observations on this boiler showed[1] that waters which cause priming produce foam on boiling. Water which is very bad produces bubbles so durable as to remain a considerable time without breaking; and by them the steam space of a boiler may be entirely filled. So soon as this takes place, instead of simply steam leaving the boiler, the discharge consists of foam, which becomes broken up in its rapid passage through the steam-pipe. With pure water, steam retains no film of liquid long enough to be seen.

(2) Water may be produced in steam from the expansions to which it is subjected. Fluctuations of pressure arise from the intermittent demand for steam, and from the steam passing from places of higher to places of lower pressure. Prof. Unwin considered it difficult to believe that any great amount of wetness arises in this way in ordinary cases.

(3) The steam in the boiler, and the steam-pipes, loses heat by radiation. Probably in some cases considerable wetness is produced in this way. The wetness of the steam, so far as it is due to this cause, will increase as the demand for steam diminishes.

The author next went on to deal with the various methods of determining the wetness of steam, referring first to the weighing method, by which a known volume is weighed, when any excess of weight above that of a corresponding volume of dry saturated steam must be due to the water present. This method is obviously one of excessive difficulty.

The superheating method was next referred to in the paper, the experiments of Barrus and Carpenter being quoted. The Carpenter calorimeter consists of a vessel about 12 inches high by 5 inches diameter, consisting of an inner chamber and a jacket. The steam from the steam-pipe passes first to the inner chamber, where the moisture is separated, and then into the outer chamber. The separating chamber is therefore perfectly protected from radiation. As the water accumulates in the inner chamber, its level is shown by a gauge glass, and the amount in hundredths of a pound can be read off on a scale. A very small orifice at the bottom of the outer chamber regulates the amount of steam discharged. The escaping steam passes through a flexible tube to a simple form of condenser. The increase of weight in any given time in the condenser is noted, and the amount accumulated in the same time in the separator.

The condensing method was next described. This is founded on the condensation of a known weight of steam and the determination of its total heat by the rise of temperature in the condensing water. By comparing the total heat per pound of a sample of steam with that of a pound of dry saturated steam according to Regnault's tables, the amount of moisture in the steam can be determined. This method was first suggested by Hirn, and the apparatus which he designed is perhaps the most

1 "Circulation in the Thornycroft Water-Tube Boiler." *Transactions of the Institution of Naval Architects*, 1894.

convenient form of apparatus for determinations by this method. It consists of an iron vessel about a foot in diameter, furnished with a loose cover; this forms the condenser. A small pipe and cock in the steam-pipe deliver steam through a small orifice near the steam-pipe into another pipe, through which it passes into the condensing water. An agitator and a sensitive thermometer are provided in the condenser. For weighing the amount of steam condensed, the whole condenser is suspended from a hydrostat, which permits extremely accurate determination of any change of weight. The hydrostat is balanced by weights till the pointer is at a fixed mark before and after condensing the steam. The condensing method, the author said, is strictly accurate in principle, but difficult to carry out in a satisfactory manner. In order to overcome these difficulties a method of continuous condensation has been introduced. By this steam and cold water are both supplied at a constant rate, and the condenser acquires a steady temperature, which can be very accurately observed. A diagram of a continuous injection condenser was shown on the wall of the theatre. Steam passes from the steam-pipe to a small injector. The condensing water is drawn from a tank, and the mixed water and condensed steam are discharged into another tank. The two tanks are placed on platform weighing machines. Two thermometers give the temperature of the condensing water (the water used for condensing the steam), and of the mixture of condensed steam and condensing water. The difference of the total weight in the two tanks, after any interval of time, is the steam condensed in that time.

A superheating method, which was introduced about the year 1890, by Mr. G. Barrus, was next referred to by the author. The steam to be tested is passed through an inner chamber jacketed by superheated steam. The sample of steam to be tested was thus dried and superheated at the expense of heat borrowed from the jacket. To avoid measuring the steam, an attempt was made to secure that equal weights of steam passed through the inner chamber and through the jacket. In that case the wetness of the steam can be calculated from observation of the temperatures only. The method, the author said, is accurate in principle, but appears to be difficult to carry out satisfactorily.

The wire drawing calorimeter was next described. This and the separating method the author considers most nearly fulfilled the necessary conditions requisite for measuring the quantity of moisture contained in steam. This calorimeter consists of two chambers, steam passing from the first to the second through an aperture $\frac{1}{16}$ inch in diameter. The full steam pressure is in the first chamber, and the pressure in the second differs little from atmospheric pressure. Thermometers give the temperatures in the chambers (which are protected from radiation), and in this way the quantity of water present in the steam is estimated. The steam is allowed to flow through the apparatus for twenty minutes or more, when the temperatures become nearly steady. No weighing is required, and temperatures only have to be observed. The observations can be continued as long as desired, so as to obtain a mean value for the dryness fraction from a considerable quantity of steam. If the steam is very wet, the temperature in the second chamber falls to about 212°, showing that wire-drawing to atmospheric pressure is insufficient to dry the steam. Practically the instrument cannot be used if the wetness exceeds the values given in the following table, the pressures being in lbs. per square inch, and the atmospheric pressure being assumed at 14·7 lbs.

| Initial pressure (absolute). | Initial pressure (gauge). | Initial temperature F. | Initial wetness per cent. |
|---|---|---|---|
| 29·9 | 15·2 | 250° | 0·80 |
| 67·2 | 52·5 | 300° | 2·44 |
| 135·1 | 123·4 | 350° | 4·21 |
| 247·7 | 233·0 | 400° | 6·13 |

Two conditions are necessary for accuracy in using this method. The second chamber must be large enough for the eddies to die out before the steam leaves the chamber. Radiation must be so far prevented that the steam in the chamber is not sensibly cooled. A calorimeter by which the separating and wire-drawing methods were combined was also explained by the author. This is the globe calorimeter which is a well-arranged apparatus.

The Cummins superheating method was also described. A vessel is filled with the steam to be tested, and then heated by a jacket. As it is heated, the rise of pressure in the inner vessel is observed, the volume being constant. So long as the steam is moist, the pressure will rise with the temperature according to the law for saturated steam. The moment all the moisture is evaporated, the rate of rise of pressure with temperature will become much slower.

The well-known salt test was next alluded to by the author. This, however, he pronounced to be inconvenient and untrustworthy, excepting perhaps in the case of a boiler subject to marked priming action.

The general conclusion drawn by the author was that the wire-drawing calorimeter without separator is the most convenient and accurate for steam with less than about 2 per cent. of moisture. For steam containing more moisture, the separating calorimeter without wire-drawing apparatus is accurate enough and convenient. The use of the separator and wire-drawing calorimeter combined is more troublesome, especially if, as is desirable, a condenser is also used to determine the amount of steam passing through the separator. In cases where there is much priming, it would seem best to take the whole of the steam through an ordinary steam-separator, measuring the amount of water trapped, and then to test by a wire-drawing or separating calorimeter the dryness of the steam after passing the separator. With priming much of the water probably flows along the bottom of the pipe, and it appears impossible that a sample can be obtained containing an average proportion of steam and water. It is recommended by Prof. Carpenter that the sample of steam to be tested should always be taken from a vertical, not from a horizontal steam-pipe. No doubt there is rather more tendency for water to flow along the bottom of a horizontal pipe than down the sides of a vertical pipe; but merely taking steam from a vertical pipe does not ensure freedom from error, especially if the amount of moisture in the steam is considerable. Variations in tests for wetness are doubtless often due to the difficulty of getting a true average sample of steam; and it would seem that errors are generally in the direction of under-estimating the amount of moisture.

A long and interesting discussion, which was adjourned from the Thursday until the Friday evening, was held on Prof. Unwin's paper. The chief point touched upon was the method to be adopted in getting a true sample of steam for analysis. This undoubtedly is the great difficulty that has to be overcome before a satisfactory method of determining the amount of moisture in steam can be arrived at. The majority of speakers were of opinion that water entrained in the steam would hang to the sides of the pipe, and a good many suggestions were made with a view to shifting the collecting nozzle over the whole area of the cross-section of the pipe, or else to give such an orifice to the nozzle as would cover a large part of the pipe area.

In his reply to the discussion, Prof. Unwin explained that this did not seem to him the true light in which the problem should be regarded. With steam rushing through a pipe at high speed, eddies would be set up which would be sufficient to thoroughly mix the steam and water so that there would be a fairly homogeneous mixture. The true difficulty arose from the checking of the velocity of the steam at the collecting orifice, an action which resulted in water accumulating so that an excess of moisture was shown in the sample drawn off. In order to overcome this, he had devised a collecting nozzle consisting of a bent-over tapered pipe, the orifice of which was at the small end, and was pointed towards the flow of steam. By adopting the necessary dimensions for the collecting nozzle, the steam collected would not be checked in velocity at the collecting orifice, and therefore moisture would not be deposited at that point.

Captain Sankey's paper was one of considerable length, and although dealing with one point only of engine design, was of great interest to engineers. It was illustrated by a large number of diagrams hung on the wall of the theatre. Without these it would be extremely difficult to give a fair idea of the course of reasoning followed by the author in discussing the merits of the two systems of governing engines. The paper, as the author said, was an elaboration of one section of a paper contributed to the Institution of Civil Engineers by the late Mr. Willans.

Speaking broadly, it may be said that the author's opinion was that the popular verdict in favour of variable expansion governing may for many purposes be accepted, yet its advantages were commonly much overrated, and in some cases it had no advantage at all.

It would be impossible within the limits of our report to trace out the respective merits of the use of the throttle valve and

automatic expansion gear, under the many conditions of working which the author supposes; and as the discussion on the paper was adjourned until the next meeting, we may leave the subject for the present.

The summer meeting of this Institution will be held in Glasgow this year, commencing Tuesday, July 30, and concluding on the following Friday.

## THE ADVANCE OF TECHNICAL EDUCATION.

THE present state of technical education in England is, on the whole, satisfactory from the scientific point of view. The authorities having the funds arising from the Customs and Excise Act under their control, are beginning to see that instruction in the principles of science is by far the most important of the requirements. They are also coming to recognise that immediate results cannot be expected from their work—that they are laying a foundation rather than erecting a complex edifice. The Technical Instruction Committees who have not sufficiently realised this, will find that they will have to materially modify their at present too ambitious schemes, postponing much of the instruction in subjects of technology until a more thorough acquaintance with the fundamental principles of science underlying all such purely technical education has been provided, for it is only by such means that the stability of their educational superstructure can be ensured.

There are no grounds, therefore, for taking a pessimistic view of the future of technical instruction. One of the most gratifying signs of development is the large number of scholarships now awarded, and the increase in the number of competitors for them. The current number of the *Record* of Technical and Secondary Education sets forth in detail a statement as to the scholarships and exhibitions actually awarded, during the year 1893-4, by County and County Borough Councils. This most valuable Return shows the number of scholarships and exhibitions awarded; the value and length of tenure of the awards; where held; conditions to be fulfilled by the candidates; the examining body, and the subjects of examination. Subjoined is a summary of the information.

| Scholarships and Exhibitions tenable at | No. of Councils. | Scholarships and Exhibitions. Number awarded. | Total yearly value. |
|---|---|---|---|
| | | | £ |
| (1) Technical, and Science and Art Schools | 36 | 3456 | 10,620 |
| (2) Secondary Schools | 37 | 1789 | 20,409 |
| (3) Universities or Institutions of University rank | 28 | 362 | 6,783 |
| (4) Short courses of instruction | 25 | 561 | 3,825 |
| | | 6168 | 41,637 |

Sixty individual counties and county boroughs are represented in the above summary. Two others, Derby and Sheffield, allocate respectively £325 and £1000 annually to scholarships, and taking these into consideration, it appears that the total sum expended for the promotion of technical and secondary education by scholarships, during the year ending March 1894, was, in round numbers, £43,000. But this by no means represents the limits of expenditure under the scholarship head. It does not take the renewal of scholarships into account, and there are still seven local authorities whose scholarship schemes have not come into operation. Also, the scholarship schemes will undoubtedly be further developed as the work goes on; in fact, it is estimated that before very long as much as £30,000 will be spent annually on scholarships by the London Technical Education Board alone. Truly, these are halcyon days for the promising young student, however humble his state of life may be.

As to the values of the scholarships, they vary from a few shillings, as a fee for a short course of instruction, to £60 a year tenable for three years. The lower limit of age is usually thirteen, and the higher, twenty-five, though we see no reason why such a maximum age should be made absolute. In some cases, the income of the parents of competitors must not exceed £400 a year, but in others—London is the most notable instance—the parents of competitors for junior scholarships must not be in receipt of more than £150 a year.

Another important statement in the current *Record* shows the plans for promoting technical and secondary education in each of the counties and county boroughs of England. From this it appears that, of the 110 local authorities in England, 96 are giving the whole, and 13 part of their grants to educational purposes. Preston is the remaining authority, and it devotes the whole of the grant available—about £1600 per year—to the relief of the borough rate. But it should be stated at once, that Preston possesses a well-endowed "Harris Institute," where technical education has been carried on for years. The total amount available by local authorities is about £744,000, of which about £144,000 is diverted to the relief of the rates, leaving £600,000 for expenditure on education. We are sanguine enough to believe that, before long, most of the £144,000 at present devoted to general county purposes will be expended in advancing technical education. London alone is responsible for £114,000 of this misapplied balance, but as its educational scheme matures, it will doubtless absorb the whole amount available. It is to be hoped that the authorities applying the remaining £30,000 to rates, will soon see how detrimental their action is to their own interests.

In this connection it is necessary to condemn the application to rates of any unexpended balance of the grant available. In every county and county borough there are persons who utterly fail to realise that the interests of science are the interests of industry. To them, immediate advantages in the shape of a minute reduction of the rates, appeals far more than prospective developments of our national industries. Had such people the control of affairs, technical instruction would indeed be curtailed within narrow limits. Fortunately, they represent but a small minority in the County Councils; nevertheless, their influence is occasionally manifest. Ever since the Technical Education Acts came into force, attempts have been made here and there to use for general county purposes the funds available and necessary for education. But if the work is to be successfully carried out, it is essential that the Technical Instruction Committees should have entire control of the grants allocated to technical education. There is far too much uncertainty about the grants even now, and the County Councils which are inclined to exercise a veto as to the destination of the surplus funds of their Technical Education Committees, will soon find that self-respecting members of the Committees will retire from the work. Recently, however, one or two Councils have shown their incapacity to understand the magnitude of the problem before them, by voting the unexpended balance of their grants to the relief of rates. This action is tantamount to declaring that the funds at the disposal of the Committee are in excess of what is required; whereas, it is hardly too much to say that additional secondary schools are needed in almost every county and county borough in England, only to mention one way in which the money might be expended. For the balance to be diverted from education is bad enough, but no great foresight is needed to see that, once the action has been taken, there is no knowing where or when it will stop. Perhaps the county of Hampshire is the most notable instance in which a County Council has crippled the work of its Technical Instruction Committee. In November last, according to the *Southampton Times*, the Council resolved to appropriate, for general purposes, £6000 from the surplus funds which had been accumulated by the Technical Instruction Committee with the idea of eventually using it in educational developments. Without taking the views of the Committee into consideration, the Council appears to have calmly confiscated the balance resulting from economical administration, and by so doing not only discouraged careful expenditure, but in one fell swoop rendered the Committee powerless to deal in the future with matters which alone could be met by exceptional means. Surely it is not too much to expect a Council to have confidence in the ability of the Technical Instruction Committee, and to allow it to know its own needs. At any rate, a Committee whose opinion is disregarded must soon lose confidence in itself. It is a matter for congratulation that County Councils generally have not treated their Committees in the same way as the elect of the county of Hampshire.

One of the most refreshing reports we have seen for some time has recently been issued by the Derbyshire County Council. The report is satisfactory, not so much on account of work accomplished, but because it affords evidence that the Committee seems to have been brought to a good understanding as to what technical instruction should mean. One of the chief difficultie

met with in most parts of the country arises from the suspicion with which the spoilt "practical man" regards the whole scheme of education designed to benefit him. No class harbours this prejudice more than farmers and their labourers. Almost every local authority complains of the apathy, or the opposition, of agriculturists to the extension of knowledge in the scientific principles of agriculture. When the Derbyshire Committee approached this branch of their work, they hesitated to establish agriculture classes under the auspices of the Department of Science and Art, because such classes are open to undesirable objections from this section of the community. In the first place, there is an appearance of attempting to teach "farming" in the lecture-room, and secondly, the teachers who are qualified to give most of the information contained in the Science and Art syllabus, are usually not actually engaged in the agricultural industry itself. Both these difficulties have been cleverly met by the Committee in this way:—"In place of the 'Agriculture' Classes of the Science and Art Department, the Committee are anxious that the students in rural evening schools shall go through a course of elementary science, which shall be of a very simple but thoroughly scientific nature. It is intended that the student shall be taught by actual experiment, and shall thus come to appreciate that the results of science are not fanciful, but are conclusions drawn from a study of actual facts. The phenomena studied in this course of lessons are of a general character, but are also largely chosen from the domain of agriculture, so that without any suspicion that the schoolmaster is attempting to teach 'farming,' the student learns a number of principles which cannot fail to affect him in practice.

"The great merits of this scheme of teaching elementary science in rural evening schools in place of starting Science and Art Department 'Agriculture' Classes are that the students are kept together year after year, studying other subjects which go to make up the curriculum, the Elementary Science course extending over two or three years, so that a first set of pupils is ready when the older ones have passed through; and further, there is no suspicion of teaching what can only be learned on the farm."

The scheme is very attractive, and good results may be expected from its application in Derbyshire. It enforces the fact that a knowledge of the elementary principles of science is the only sound basis upon which to build courses of technology.

The county of Derby is more dependent upon the mining industry than on any other; therefore its organisation of instruction in mining deserves a word of remark. It attempted to provide the instruction by means of courses of lectures delivered in a certain number of pit villages, but the results were hardly successful. The teaching was afterwards given by local men who had practical knowledge of mining, and some acquaintance with collateral branches of science, and this scheme was more satisfactory than the former. The point to be borne in mind in all such cases is that chemistry, steam, geology, some branches of physics, and mechanical drawing should form a part of the education of every mining student. With reference to local teachers, a word may be necessary. There is, of course, a tendency in many districts to patronise local ability, but it should always be borne in mind that the local man is not of necessity the man who will do best. In the expenditure of public money, it ought to be a guiding principle that the best teachers available by advertisement and good salaries should always be selected.

The evergreen complaint of Technical Education Committees finds expression in the report from Derbyshire. In connection with the subject of evening continuation schools, we read: "The Committee have found that one of the great difficulties which the ordinary student experiences in receiving instruction in every kind of technical subject is the lack of sufficient preliminary knowledge. His elementary school education has very largely leaked away, instead of having been continued to the point giving easy comprehension of scientific principles and problems. This implies that the national expenditure on elementary education is very largely wasted without some supplementary scheme by which the instruction given shall be conserved and continued until the student is old enough to grasp its importance." This puts the whole matter in a nutshell. When the average boy leaves the elementary school, at about fourteen years of age, he regards his education as completed, with the result that he has no interest whatever in schools or classes of any educational value. Committees must not hope to attract the majority of working lads into evening continuation schools, however diversified their prospectuses may be. Only here and there can pupils be found who have begun to see the depths of their ignorance, but these are the minds to nurture, and for their benefit systems of evening-classes should be constructed.

Before the era of the County Councils, the principal means for the spread of instruction in elementary science was undoubtedly the classes of the Science and Art Department. Testimony is borne to this in the report referred to. We read:

"It has been the custom in many educational quarters to criticise and condemn the methods of that Department, and to create an impression that but little good has been accomplished. The contrary of this is undoubtedly the case. In all manufacturing centres it will be found that there are numbers of persons applying to every kind of industry scientific principles and knowledge gained at Science and Art Classes, and which could not have been gained anywhere else during the last thirty-five years." The Department is very well able to take care of itself, but this statement of fact may be profitably considered by those who disparage its usefulness.

If there is one thing our educational system lacks more than another, it is proper facilities for secondary education. Recognising that a good education in a secondary school is the only means by which the highest branches of technical instruction could be approached, the Derbyshire Committee offered for competition sixty scholarships tenable at secondary schools. After the awards had been made, it was found that only six out of the sixty successful candidates chose schools in the county, and that there was only one school in the administrative county available for the girls who had won scholarships. An inspection of the Grammar Schools, and similar institutions in the county, with a view to determining their educational conditions and needs, revealed a general want of proper equipment; indeed, only one out of nine secondary schools had a chemical laboratory. Derbyshire is certainly not alone in this deplorable state of things, which it will take some years to improve. The fact that the defects in our educational system are being exposed, and that attempts are being made to meet them fairly, is a clear promise of progress. If the Technical Instruction Committees had done no more than reveal the inefficiency and insufficiency of scientific instruction in the counties of England, they would have furthered the interests of science. But as they have also helped to organise and increase the facilities for such instruction, they have worked in no mean way for the extension of natural knowledge. Encouragement and friendly guidance is all that is required to render the work even more valuable in the future than it has been in the past.

R. A. GREGORY.

## SCIENCE IN THE MAGAZINES.

FIRST in importance among the contributions to the February magazines is a collection of opinions on forest preservation, published in the *Century*. In response to a request from the editor of that magazine, a number of persons interested in arboriculture sent their views as to the general need of a thorough, scientific, and permanent system of forest management in the United States, and specifically as to the plan suggested by Prof. C. S. Sargent, which comprised the following features:—

(1) Forestry instruction at West Point; the establishment of a chair of Forestry at the United States Military Academy, to be supplemented by practical study in the woods and by personal inspection of foreign systems of forestry.

(2) An experimental forest reservation; the purchase on the Highlands near West Point, or elsewhere, of a small territory for the use of the proposed new branch of instruction.

(3) Control by educated officers; the assignment of the best educated of these officers to the supervision of the forest reservations.

(4) The enlistment of a forest guard; a body of local foresters, to be specially enlisted for the purpose of carrying out the principles of forestry thus taught.

The experts consulted agree in the opinion that the United States needs a thoroughly scientific and permanent system of forest management in the interests of the people of to-day, and of future generations. But the general feeling seems to be that the management of the forests should not be placed under any military organisation. As to the suggestion to increase the curriculum of the U.S. Military Academy so as to cover

instruction in forestry, it is rightly pointed out that a course in forestry which is merely an adjunct to a military education must fail to produce the highest efficiency as foresters in the officers who take it. "Adequate training in so large a subject," is remarked by one of the correspondents, "can be reached only by long and undivided attention." And further, as Prof. Cleveland Abbe says, "the arts of warfare are a special application of the arts of peace, and it is a perversion of the military school to make it a rival of the civilian schools of engineering, chemistry, forestry, &c." The general opinion appears to be that for the preservation and management of forest reservations, a permanent body of foresters is required, but it should be composed of practical woodmen who have devoted some years to the study of forestry. If we may express an opinion upon the matter, to us this seems the only view which can have any support from scientific men. To make forestry a subsidiary branch of a military education, would be to establish scant provision for the science. If merely the protection of forest reservation be aimed at, Prof. Sargent's plan probably offers the easiest way of obtaining it; but if the forests are to be developed, a forest school, where foresters can be scientifically trained, becomes essential.

Another contribution to the *Century* is the sad story of the death of Emin Pasha, told by Mr. R. Dorsey Mohun, the United States agent in the Congo Free State. In April of last year, Mr. Mohun arrested and took the confessions of the two Arab slaves, named Ismailia and Mamba, who had killed Emin. The following is a brief statement of the melancholy facts:—In the Unyoro country, to the west of the Victoria River, Emin came upon an Arab camp, under the command of Said ben Abedi. He expressed his intention of making his way to Kibonge, about eighty miles south of Stanley Falls, and it was arranged that his force, numbering about 150 people, and Said's, should travel together. On October 5, 1892, Emin and Said arrived at the small village of Kinena, which lies 150 miles to the north-east of Kibonge. Said then went on to inform the Kibonge chief that the white man was coming, Ismailia and Mamba going with him. About twenty days later, Mamba returned with a letter to Emin, saying that safe conduct to Kibonge should be given; but the Kibonge chief sent another letter to the Kinena chief by Ismailia, containing instructions to kill Emin. The Pasha was induced to send his men into the plantations on a pretext, and while they were away he was murdered, and his head sent to Kibonge. This appears to have occurred on the morning of October 28, 1892. Emin's head was sent by Kibonge to Munie-Mohara at Nyangwe, the reason being, Mr. Mohun thinks, that Kibonge wished to show that he could kill a white man as well as Munie-Mohara, who had ordered the destruction of Hodister's expedition five months previously. "Not the slightest suspicion," says Mr. Mohun, "attaches to Said ben Abedi of having had any connection with Emin Pasha's death, which is regarded by the Arabs with whom I have talked as a stupid error on the part of Kibonge, who committed the crime simply to place himself on the same level as Munie-Mohara, who had killed Hodister. I do not believe, either, that Tippoo Tib had any hand in the crime, which must have been as great a surprise to him and to his son, Sefu, and his nephew, Rachid, who was the Governor of Stanley Falls, as it was to us." An article on "New Weapons of the United States Army," also in the *Century*, and the eighteen pictures and diagrams which illustrate it, will interest many of our readers.

Dr. Charles L. Dana writes on "Giants and Giantism," in *Scribner*. Two years ago a man nearly seven feet in height, possessing very large feet, hands and head, came under his notice, and was found to be a victim of the peculiar disease known as acromegaly. The man died from the effects of his disease, and a portion of the brain—the pituitary body—was then found to be enlarged to many times its original size. This gave support to the idea that enlargement of the pituitary body is the cause of the gigantic growth of the extremities in acromegaly, and that giants generally are not simply freaks, but victims of a nervous disorder. The skeleton of the famous Irish giant was studied some time ago by Prof. Cunningham, and found to be characteristic of a case of acromegaly, and an examination of photographs of nearly all the living giants now on exhibition leads Dr. Dana to believe that about one half of them are acromegalics. According to Dr. Dana, "extraordinary size is a disease, a neurosis of nutrition, rather than a chance disturbance of development. . . . It is possible by certain kinds of gland-feeding, to increase the stature of dwarfed persons very rapidly. There is, for example, a gland called the 'thyroid body' lying in the neck, the juice of which, when fed to certain kinds of dwarfs (coctins) causes them rapidly to grow. Experiments in feeding animals and men with the pituitary body are now in progress."

"The Method of Oganic Evolution" is expounded by Dr. A. R. Wallace in the *Fortnightly*, the article being really a critical and adverse review of Mr. Bateson's views on discontinuous variation, as set forth in "Materials for the Study of Variation." A second article on discontinuity in evolution, dealing with the theories advanced by Mr. Galton, will appear in a future number.

A posthumous essay, by Dr. G. J. Romanes, entitled "Longevity and Death," appears in the current number of *The Monist*, to which it was sent by Prof. C. Lloyd Morgan. In it an unpublished essay, written in 1875, is quoted, in which occur the following passages:—

"Those species whose ancestral types have frequently been required to vary would have gained much during the history of their descent, by having their constituent individuals short-lived; for in this way a comparatively great number of opportunities would have been afforded for the requisite variation to arise: in other words, a comparatively great number of variations would have occurred in a given time. Hence it seems natural to infer that it is in the power of Natural Selection to affect the *curtailment* of individual life, wherever such curtailment would be of advantage to the species, that is to say, wherever *flexibility* of type is required. Of course, length of life is not the only factor which determines flexibility of type. There are at least three other such factors: (1) the period at which puberty sets in, (2) the number of times the individual breeds during its life-time, and (3) the number of young which it bears at each time of breeding. Nevertheless, it is true that the length of life is a highly important factor, because, if the individual is short-lived, it becomes a necessary condition to the continuance of the species that parturition should be frequent. Or, more generally, there must be more or less of a direct proportion between the potential longevity of every species and the frequency of parturitions characteristic of that species—if not also of the number of offspring in each. Now, as Mr. Lankester has pointed out, there is, as a matter of fact, a highly remarkable correlation between potential longevity in the individual and frequency of parturition, as well as of numbers constituting the litter which are distinctive of the species. This correlation he attributes to generative expenditure acting directly to the curtailment of life; but in holding this view, I suspect that he is mistaking cause for effect. I do not think it is generative expenditure which causes curtailment of life, but that it is curtailment of life by Natural Selection which causes the high generative expenditure within the lessened period. It is as though all the conditions needed to secure flexibility of type were adaptively associated in these species which have survived in a comparatively fluctuating environment. Moreover, it is worth observing that all the organisms to which Mr. Lankester ascribes a practically unlimited potentiality of life, are organisms which, as far as we can judge, must always have been exposed to uniform conditions of life."

In addition to this, *The Monist* contains an article by Dr. E. Montgomery, who attempts "first to gain a scientifically justified and logically consistent physical basis, upon which a naturalistic conception of vitality can be reared; and then, to show to what special physical conditions vital activities and vital organisations owe their existence." We also notice a metaphysical paper on "The Natural Storage of Energy," by Mr. Lester F. Ward.

Pascal is the subject of a paper, by the late Mr Walter Pater, in the *Contemporary*, but his scientific researches are not dealt with. Towards the end of the notice, there is a note on the influence of imagination on his work. It is: "Hidden under the apparent exactness of his favourite studies, imagination, even in them, played a large part. Physics, mathematics, were with him largely matters of intuition, anticipation, precocious discovery, short cuts, superb guessing. It was the inventive element in his work and his way of putting things that surprised those best able to judge. He might have discovered the mathematical sciences for himself, it is alleged, had his father, as he once had a mind to do, withheld him from instruction in them." A bright and sensible paper on "Nervous Diseases and Modern

Life" is contributed to the same review by Prof. Clifford Allbutt. There is also a paper of interest to physical geographers, its title being "The Evolution of Cities."

The current *Quarterly Review* has among its articles a sketch of the history of the Ordnance Survey, wherein we read "the scope of the undertaking exceeds any programme heretofore attempted by any Government, the mode and style of its execution are second to none, either from a scientific, artistic, or utilitarian point of view, and the cost of the work, stroke for stroke, is probably lower than that paid by any other nation for a similar purpose." Prof. Huxley's collected essays, and other works, are reviewed under the title "Prof. Huxley's Creed," and in the article "England in Egypt," the irrigation of Egypt, and the construction of the Philæ dam, are noticed.

The *Reliquary and Illustrated Archæologist* is rich in good illustrations. Among the articles we notice an account of the exploration of a Hunnish cemetery at Czika, near Buda-Pesth. Parts of a number of skeletons have been found, and a complete skeleton of a woman, six feet three inches in length. Weapons, stirrups, earthenware vessels, and various ornaments have also been found. "The Burning of the Clavie," a ceremony still carried out on the last night of the old year at Burghead, in the north of Scotland, is described by Mr. H. W. Young. The custom appears to have come down from the most remote ages. The natives of Burghead assert that it is a Druidical worship, while Mr. Young believes it to be simply a revival of the worship of Baal—a remnant of that great fire worship which prevailed over the whole world as known to the ancients. In the notes is an illustration of the ancient Egyptian tomb in the island of Elephantina, discovered and explored by H. R. H. the Crown Princess of Sweden and Norway, and an illustrated description of the re-erection of those interesting pre-historic monuments, the Dartmoor menhirs. Recent investigations have yielded some evidence which connects these stone-rows with the Neolithic period.

A passing notice must suffice for the remaining articles on scientific subjects in the magazines received by us. *Good Words* contains Sir Robert Ball's concluding paper on "Sir Isaac Newton," and a brief paper "On the Anti-toxin Cure for Diphtheria," by Dr. W. J. Fleming. A visit to the tomb at Dashur, where the jewels of an Egyptian princess of the Twelfth Dynasty were found last year, is described by Mrs. St. Loe Strachey in the *National*. The *Humanitarian* has an article on "The Prevalence of Nervous Diseases," by Dr. S. Althaus. "Some Curiosities of Modern Photography" are brought together by Mr. W. G. FitzGerald in the *Strand Magazine*. The illustration he gives of an image photographed through the eye of a beetle is, however, quite eclipsed by a photograph taken by Dr. Spitta through the lenses of the composite eye of a water-beetle, and reproduced in *Knowledge* for July 1894. Mr. Grant Allen contributes a rhapsody on quails to the *English Illustrated*. *Chambers's Journal* contains its usual complement of readable articles on scientific topics.

## *UNIVERSITY AND EDUCATIONAL INTELLIGENCE.*

OXFORD.—In a Congregation held on Tuesday, February 12, the amendments to the proposed Statute on Research Degrees came under consideration. There were sixty-three amendments, and of these only fourteen came under consideration, as the debates on some of them were of some length. The first amendment, proposed by the Provost of Oriel, and seconded by Prof. Odling and Mr. Strachan Davidson, proposed that the Degree of Bachelor of Arts should be substituted for the proposed Degrees of Bachelor of Letters and Bachelor of Science. After a prolonged debate the amendment was negatived by 137 votes against 33.

An amendment by Prof. Case, defining "Science" as including Mathematics, Natural Science, Mental and Moral Science, was carried by 137 votes against 34. An amendment, proposed by Mr. C. Cannan, of Trinity College, and seconded by Mr. Bourne, provided that the supervision of the candidates for Research Degrees should be vested in the Boards of Faculties, instead of in a special Delegacy as proposed by the Statute under consideration. This amendment was carried by 110 votes against 49. Another amendment, by Prof. Holland, which proposed that candidates for Research Degrees, not being already Graduates of the University, should have obtained a degree in some other University, was rejected by 107 votes against 39. The other amendments were either consequential on those already mentioned, or were of a formal character. The further consideration of the amendments was fixed for Thursday, February 21.

CAMBRIDGE.—The Sedgwick Prize in Geology has been awarded to Mr. Henry Woods, of St. John's College, Demonstrator in Palæontology. The subject proposed for the prize of 1898 is "The Glacial Deposits of East Anglia." The essays are to be sent to the Registrary by October 1, 1897. Candidates must be Graduates of the University who have resided sixty days during the preceding twelve months.

Mr. M. Laurie, of King's College, has been appointed by the Special Board for Biology and Geology, to occupy the University's table in the Naples Zoological Station, for three months from March 1.

A course of lectures in Anthropology, with practical work, is announced by Prof. Macalister for the Lent and Easter Terms. The lecturer is Prof. A. C. Haddon, of the Royal College of Science, Dublin. The subject of the first lecture, on February 14, at 3.30, is "The Methods of Anthropology."

The degree of Sc.D. *honoris causa* is to be conferred on Sir William MacGregor, Administrator of British New Guinea, in recognition of his able contributions to anthropology and ethnography.

The following appointments of electors to Professorships in Natural Science and Medicine are announced. Chemistry, Dr. T. E. Thorpe; Plumian of Astronomy, Dr. A. R. Forsyth, and Mr. W. H. M. Christie, Astronomer Royal; Anatomy, Dr. Allbutt; Botany, Mr. A. Sedgwick; Geology, Prof. Newton; Jacksonian of Natural Philosophy, Lord Rayleigh; Downing of Medicine, Dr. A. Macalister; Mineralogy, Prof. J. J. Thomson; Zoology, Dr. D. Macalister; Cavendish of Physics, Lord Rayleigh; Mechanism, Prof. Osborne Reynolds; Physiology, Mr. J. N. Langley; Surgery, Dr. A. Macalister; Pathology, Dr. Gaskell.

A grant of £50 from the Worts Travelling Scholars Fund has been made to Mr. P. Lake, of St. John's College, for the purpose of investigating the distribution of Trilobites in Russia and Sweden.

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A PARLIAMENTARY PAPER dealing with the moneys received by the Councils of Counties and County Boroughs in England and Wales under the Local Taxation (Customs and Excise) Act, 1890, and available for technical education, has just been published. The following summary shows how the moneys have been expended:—

| | Counties (other than London) and County Boroughs. | County of London. | Total. |
|---|---|---|---|
| | £ | £ | £ |
| Aggregate amount received up to March 31, 1894 ... | 2,439,319 | 687,034 | 3,126,353 |
| Aggregate amount expended on— | | | |
| (*a*) Technical and Intermediate Education ... | 1,481,712 | 27,246 | 1,508,958 |
| (*b*) Purposes other than Technical and Intermediate Education ... | 290,508 | 600,034 | 890,542 |
| Aggregate amount appropriated to Technical and Intermediate Education, but remaining unexpended at the date of the Returns ... ... ... | 635,933 | 59,754 | 695,687 |
| Residue not appropriated for Technical and Intermediate Education, but remaining unexpended at the date of the Returns ... ... ... ... | [1] 31,166 | — | 31,166 |
| | 2,439,319 | 687,034 | 3,126,353 |

1 £6700 of this amount had been appropriated to County buildings and museum.

Seventy-one out of the 126 Councils had expended on, or appropriated to, Technical or Intermediate Education the whole of the moneys they had received from the residue of the Beer and Spirit Duties.

## SOCIETIES AND ACADEMIES.

### LONDON.

**Physical Society**, February 8.—Annual General Meeting.—The chair was taken by the retiring President, Prof. Rücker.—The Treasurer, Dr. Atkinson, presented his report for the year 1894. The balance-sheet showed a somewhat larger expenditure than in previous years, the increase being partly due to the new system of publishing abstracts, partly to the rent of rooms and the expenses of tea. The balance in the bank had increased by about £33 during the year; but the Treasurer said that, strictly speaking, the Society had trenched on its capital to the extent of about £30, and that this would probably be the last report for some time to come which would show a balance in favour of the Society. The assets of the Society exceeded its liabilities by £2642 0s. 5d. Prof. Carey Foster asked whether it would not be possible, in estimating the assets of the Society, to make some allowance for the stock in hand. Dr. Atkinson replied that that had not hitherto been done, and the difficulty would be to assign a money value to the stock. The stock of the works of Joule, and of other memoirs, was of course decreasing, while the sale of the *Proceedings* was becoming somewhat greater. As regarded the securities of the Society, their actual value would be about £200 or £300 more than appeared on the balance-sheet. Prof. Rücker said that the Society had deliberately entered upon a policy of expansion, and that they must be prepared to find the expenditure increasing. On the other hand, it was hoped that by making the Society more attractive, a greater number of persons would be induced to join. In view of the great advantages now enjoyed by members, there had been some suggestion of raising the subscription; but, in any case, he thought that they might look forward to the future with confidence. The report was then moved and adopted.—The next business was the election of Officers and Council for the year 1895-6, and Messrs. Rhodes and Yule, being asked to act as scrutators, collected the balloting lists.—Prof. Carey Foster proposed a vote of thanks to the Lords of the Committee of the Council on Education, for having allowed the Society to meet at the Royal College of Science. At the commencement of the life of the Society, its founder and first President, Guthrie, had obtained permission for the meetings to take place at South Kensington, and the Society had continued to meet there until their recent migration to the rooms of the Chemical Society. The vote of thanks was duly seconded and was carried unanimously. Major-General Festing then proposed, and Mr. Croft seconded, a vote of thanks to the auditors, Messrs. Inwards and Trotter. This also was carried. Mr. Trotter then proposed a vote of thanks to the retiring Council; they had shown an energy which was rare in such societies, and had inaugurated an active and original policy, which must prove of the greatest benefit to the Physical Society and to physical science generally. Carried unanimously.—Mr. Elder gave notice of a proposed alteration of the rules, the object being to allow the Council under certain conditions to admit persons into the Society without requiring from them the usual number of recommendations from members. It was pointed out that sometimes eligible persons, especially those resident abroad, were unable to enter the Society because they were unknown to any of the existing members. The motion to sanction the proposed alteration was put from the chair and carried, but this decision will need to be confirmed at a subsequent meeting, of which due notice will be given.—Mr. Rhodes then read the following list of the Officers and Council elected for the year 1895-6: President, Capt. Abney. Vice-Presidents who have filled the office of President: Dr. Gladstone, Profs. Carey Foster and Adams, Lord Kelvin, Profs. Clifton, Reinold, Ayrton, Fitzgerald, Rücker. Vice-Presidents: Mr. W. Baily, Major-General Festing, Prof. Perry, Dr. Stoney. Secretaries: Messrs. Blakesley and Elder. Treasurer: Dr. Atkinson. Demonstrator: Prof. Boys. Other members of Council: Mr. Shelford Bidwell, Mr. W. Crookes, Messrs. Fletcher, Glazebrook, G. Griffith, Profs. Henrici, Minchin, Mr. Swinburne, Profs. S. P. Thompson and S. Young.—Prof. Rücker then vacated the chair in favour of Captain Abney, and the meeting being resolved into an ordinary meeting, Mr. W. B. Croft gave "an exhibition of simple apparatus." An optical bench was shown which consisted of a wooden lath of rectangular section, furnished with a millimetre scale, and clamped on to the table, together with three flat wooden blocks, whose contacts with the table and the lath left them only freedom to slide in a direction parallel to the scale. Another apparatus was designed for observing anomalous dispersion. A cork supported two rectangular pieces of microscope cover-glass, which were inclined at a small angle to one another; and a drop or two of a strong alcoholic solution of fuchsine being introduced between them was maintained in position by capillary action. Photographs were shown of Chladni's sand-figures, some of the forms being of an unusual character. Mr. Croft also exhibited a polariscope in which the polariser was a thin piece of glass stuck on to cork by means of black sealing-wax, and the analyser a plate of tourmaline; as well as a miniature model of Grove's gas battery. Photographs of some curious optical phenomena were projected on the screen, including 12-rayed stars seen on looking at a bright source of light through certain specimens of mica, and pairs of intersecting or non-intersecting circles of light, obtained under similar circumstances with (doubly-refracting) fibrous calcite. These last, it was suggested were similar in origin to the curves obtained by reflection at, or transmission through, a diffraction-grating held obliquely. Clock-springs broken by frost were also exhibited, each spring having given way in a very great number of places simultaneously. Dr. Johnstone Stoney said that many years ago he had published in the *Transactions* of the Royal Irish Academy an investigation of the circles seen in fibrous calcite, and had shown geometrically that they had nothing to do with the regularity of the fibrous structure, but were due to reflection and refraction within the crystalline plate. The distribution of the planes of polarisation round the circumferences of the circles was also accounted for by his investigations. Mr. Price said he had found that when a clock-spring during the process of hardening was kept in shape by wires, subsequent fracture was most apt to occur at those places where the wires had been in contact with the spring.—Mr. Rhodes asked if Mr. Croft had ever tried Newton's experiment of admitting sunlight between two sharp edges inclined at a small angle to one another. He had not been able to obtain the hyperbolic bands described by Newton. Mr. Croft said he had not tried the experiment exactly in that form. Captain Abney said that this experiment had succeeded very well in his hands.—Mr. S. Skinner read a paper on the tin chromic chloride cell. He said that his attention had been attracted to the cell by an account published by Mr. Case, of New York. The cell had been stated to give no E.M.F. at ordinary room-temperatures, while it gave a considerable E.M.F. at 100° C. The author had found that when the cell was directly connected up to a galvanometer, there was no current at ordinary temperatures, and some current at 100° C.; but when he had measured the E.M.F. by Poggendorff's method, he had found ·44 volt at 15° C. and ·40 volt at 97° C. The cell as originally described consists of a tin plate and a platinum plate immersed side by side in a solution of chromic chloride; when the temperature of the cell is near to 100° C., and the poles are connected, the following reaction occurs:—

$$Cr_2Cl_6 + Sn = 2CrCl_2 + SnCl_2;$$

and when the poles are disconnected and the cell cooled, the reverse change takes place. The author prefers to use as electropositive metal an amalgam of tin and mercury instead of a tin plate, so that when the tin precipitated during cooling falls to the bottom of the solution, it is redissolved in the mercury, and the cell has regained its original state. When silver nitrate solution is added to chromic chloride, only two-thirds of the chlorine comes down as silver chloride, and this has led the author to suppose that the proper formula for chromic chloride is $CrCl \,.\, Cl_2$. Hence he works out the electrolytic action by means of a Grotthus chain. Prof. Rücker asked whether a change of polarisation would explain the behaviour of a cell at different temperatures. Prof. Carey Foster asked whether the reversed chemical action on cooling from a high temperature were accompanied by a reversed E.M.F. Mr. Skinner said no. The tin was precipitated throughout the solution, and not at the surface of the tin plate, so that no E.M.F. of the kind was to be expected. Mr. Appleyard thought that Prof. Minchin had used tin chloride cells with two tin plates for electrodes, the

cells only working when one plate was illuminated. Mr. Trotter wished to know whether heating the cell supplied energy to it, or simply removed an obstacle in the form of polarisation. Mr. Skinner thought that heating acted by removing an obstacle. Captain Abney: And so doing work.

PARIS.

**Academy of Sciences**, February 4.—M. Marey in the chair.—The proceedings were commenced by the announcement of the decease of Prof. Arthur Cayley, correspondent of the Astronomy Section from 1863. M. Hermite then gave a short account of the scientific work of the great mathematician.—On argon, a new constituent of the atmosphere, discovered by Lord Rayleigh and Prof. Ramsay. An abstract by M. Berthelot of matter that has already appeared in these columns. He concludes with the observation that, although argon has no action on the higher animals, it cannot be predicted that bacteria may not be affected by it, for it is known that they absorb nitrogen. The suggestion is made that nitrogen obtained from the total destruction of animal or vegetable matter should be examined for argon.—On abelian functions, by M. H. Poincaré.—Propellers with tangential penetration, by M. Guyou. A description of a model of a new type of propeller.—The present state of investigations on the vegetation of French colonies and protectorates, by M. Ed. Bureau.—On a passage of the shadow of Jupiter's fourth satellite, by M. J. J. Landerer. The calculated time of half-passage is 27m. 9s.; observation gives the time as 21m. 30s.—Solar observations made at Lyons Observatory during the fourth quarter of 1894, by M. J. Guillaume. There appears to have been a general decrease of spots and faculæ since the first quarter of 1893.—On continuous straight beams rigidly connected with their supports, by M. Eugène Laye.—On the nature of the "displacement current" of Maxwell, by M. Vaschy. A mathematical paper in which the author shows that certain properties admitted by Maxwell as hypotheses are mathematically exact, and may be deduced from his theory.—On the anomalous rotatory dispersion of crystallised absorbent media, by M. G. Moreau.—On Fresnel's biprism, by M. Georges Meslin.—Influence of low temperatures on the attractive power of artificial permanent magnets, by M. Raoul Pictet. The tabulated mean results of four sets of experiments, agreeing well together, show a continuously increasing attractive power as the temperature becomes lower. The range of temperature in these experiments was from + 30° to − 105°.—Moniodammonium derivatives of hexamethyltriamidotriphenylmethane, by M. A. Rosenstiehl. Methyl iodide forms with the complex triamines $A_3{:}C.R$ (where A represents $(CH_3)_2.N.C_6H_4$, and R indicates an electropositive radical such as H, OH, or $OCH_3$) two series of colourless combinations. (1) The first contains one atom of pentavalent nitrogen. Compounds of this class exchange the radical R for an acid radical, and form colouring matters. (2) The second series, formed by the addition of three molecules of methyl iodide, contains three pentavalent nitrogen atoms. Colouring matters are not formed in this case, as the radical R cannot be exchanged for an acid radical.—On laccase and the oxidising power of this diastase, by M. G. Bertrand.—Reactions of chelidonine with the phenols in sulphuric solution, by M. Battandier. With a drop of guaiacol dissolved in 0·5 c.c. of concentrated sulphuric acid, chelidonine moistened and exposed to the air gives a fine carmine colouration. A list of colour reactions with other phenols is given.—On the development of the body in the shrimp (*Palemon serratus*, Fabr.) and the crayfish (*Astacus fluviatilis*, Gesn.), by M. Louis Roule.—On the production of females and males among the Meliponas, by M. J. Perez.—On the influence of climatological conditions on the growth of trees, by M. Émile Mer. The author shows that the dryness of 1893 caused a diminution in the growth of wood in forest trees. A similar effect was produced by the excessive rainfall of 1888.—On refraction in polychroic aureoles, by M. A. Michel Lévy. When the aureoles are well developed, with free contours, deeply coloured, the refraction of the pigmented part is clearly superior to that of the unmodified substance. The difference between the similar indices of refraction may amount to a decimal of the second order. It follows that the constitution of the mineral is profoundly modified, as its ellipsoid of optical elasticity is different.—On the existence of numerous remains of sponges in the *phtanites* of the Precambrian of Brittany, by M. L. Cayeux. Conclusions: (1) There exist, at the base of the Precambrian of Brittany, numerous and varied sponge spicules. (2) Almost all, if not all, the orders of sponges with siliceous skeletons are represented at this early epoch.—On the existence of a submarine delta in the Upper Cretaceous near Châtillon-en-Diois, by MM. G. Sayn and P. Lory.

## BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—The Foundations of Belief: A. J. Balfour (Longmans).—Cellulose: Cross and Bevan (Longmans).—Thoughts on Religion: Dr. G. J. Romanes (Longmans).—Elementary Lessons in Electricity and Magnetism: Prof. S. P. Thompson, new edition (Macmillan).—Remarkable Comets: W. T. Lynn, 3rd edition (Stanford).—Varied Occupations in Weaving: L. Walker (Macmillan).—Theoretical Chemistry from the Standpoint of Avogadro's Rule and Thermodynamics: Prof. W. Nernst, translated by Prof. C. S. Palmer (Macmillan).—Atlas of Classical Antiquities: Th. Schreiber, edited for English Use by Prof. W. C. F. Anderson (Macmillan).—Annuaire de L'Observatoire Royale de Belgique, 1895: F. Folie (Bruxelles, Hayez).—Alembic Club Reprints; No. 10, Researches on the Arseniates, Phosphates, and Modifications of Phosphoric Acid: T. Graham (Edinburgh, Clay).—Die Bearbeitung des Glases auf dem Blasetische: D. Djakonow and W. Lermantoff (Berlin, Friedländer).—A Treatise on Industrial Photometry with Special Application to Electric Lighting: Dr. A. Palaz, translated by G. W. and M. R. Patterson (Low).—Bulletin of the U.S. Fish Commission, Vol. xii. (Washington).—Chemical Laboratory Labels, Part 2: W. H. Symons (Gallenkaemp).—Brasilische Pilzblumen: A. Möller (Jena, Fischer).—Elliott Brothers' Catalogue: No. E4 (Elliott).—A Fisherman's Fancies: F. B. Doveton (Stock).—Calendar of the Department of Science and Art, 1895 (Eyre and Spottiswoode).

PAMPHLETS.—On the Present Relations of Agricultural Art and Natural Science: Prof R. Warington (Frowde).—Bibliography of Aceto Acetic Ester and its Derivations: P. H. Seymour (Washington).—Geology of the Boston Basin: W. O. Crosby, Vol. 1, Part 2 (Boston).—North American Bows, Arrows, and Quivers: O. T. Mason (Washington).—Quelques Aperçus sur l'Esthétique des Formes: C. Henry (Paris, Nony).

SERIALS.—Quarterly Journal of the Geological Society, Vol. li. Part 1, No. 201 (Longmans).—Transactions of the English Arboricultural Society, Vol. 3, Part 4 (Carlisle).—Psychologische Arbeiten, Erster Band, 1 Heft (Leipzig, Engelmann).—American Journal of Mathematics, Vol. xvii. No. 1 (Baltimore).—Bulletin of the American Mathematical Society, January (New York, Macmillan).—Bulletin de la Société Impériale des Naturalistes de Moscou, 1894, No. 3 (Moscou).—Proceedings of the Physical Society of London, Vol. xiii. Part 3 (Taylor and Francis).—Proceedings of the Academy of Natural Sciences of Philadelphia, 1894, Part 2 (Philadelphia).—Bulletin of the Buffalo Society of Natural Sciences, Vol. v. No. 4 (Buffalo).—Bulletin de L'Académie Impériale des Sciences de St. Pétersbourg, January (St. Pétersbourg).—U.S. Department of Agriculture (Division of Chemistry), Bulletins Nos. 41–43 (Washington).—Proceedings of the Boston Society of Natural History, Vol. xxvi. Parts 2 and 3 (Boston).—American Journal of Science, February (New Haven).—Medical Magazine, February (Strand).—Journal of the Academy of Natural Sciences of Philadelphia, 2nd series, Vol. x. Part 2 (Philadelphia).—Strand Magazine, February (Newnes).—Strand Musical Magazine, February (Newnes).—Picture Magazine, February (Newnes).—Science Progress, February (Scientific Press, Ltd.).—Bulletin of the Geological Institution of the University of Upsala, Vol. 1, Nos. 1 and 2 (Upsala).

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